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ART. I.—CASES OF PERICARDITIS MASKED BY DELIRIUM.

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In the winter of 1849–50, a case of pleuro-pneumonitis, complicated with pericarditis, and accompanied by a peculiar form of delirium, the local affection, so far as concerns the rational symptoms, being remarkably latent, came under my observation;—a report of which was published in the Buffalo Medical Journal.* The fact that pericarditis is occasionally so masked by cerebral symptoms that the latter (without knowledge of this fact) are likely to engross the attention of the practitioner, leading him to regard the affection as seated within the head, has but recently been pointed out. Dr. WATSON, in his printed lectures on practical medicine, speaks impressively of this fact. But in the late work of Dr. GEORGE BURROWS, on disorders of the cerebral circulation, is a fuller consideration of the subject. Dr. B. gives an account of its literature, adducing a number of cases which have been communicated, within a few years past, by different writers. In connection with the report already referred to, the reader will find extracts from that portion of Dr. BURROW's work which pertains to the subject.

* No. for April, 1850. Vol. V., page 505.

Since that report was made, two cases illustrating the coincidence of delirium and pericarditis have fallen under my observation.

One of these cases was observed at the Buffalo Hospital of the Sisters of Charity, in 1851. The notes of this case, at the time I am writing, are not available, and an account of it must therefore be given from memory. It will not be difficult to do this sufficiently for all practical purposes, as the patient was seen by me but once. At my morning visit at the Hospital, I found that, on the evening previous, a patient had been admitted greatly prostrated, and in a state of delirium. Nothing was obtained relative to the previous history of the case. The patient had not spoken since his admission. He lay with his eyes open, fixed, most of the time, in one direction, taking no notice of things or persons around him, making no reply to questions. A peculiarity in this case was, that the patient frequently ejected saliva with force and without any regard to its destination. His bed, and the floor in proximity thereto, were covered with spittle. Persons in proximity to him were in danger of receiving it on their persons, not from design, but because it was scattered at random, the patient not changing his position, but lying constantly on the back. Under these circumstances I deferred an examination of the case, and at my next visit I found the patient had died. The circumstances pertaining to the mode of dying I am unable to state from recollection. At the time I was observing the patient, the idea of pericarditis did not occur to me, but in thinking of the case afterward, a resemblance in the character of the delirium to that in the case of Crotty, formerly reported by me, led me to suspect this disease; so that, before the autopsy was made, I ventured to predict that it would be discovered. My prediction proved to be true. Recent pericarditis existed, and the heart was preserved as a specimen of that affection. With respect to the condition of other organs, I am unable to state without reference to the record made at the time.

Another case, illustrating the occurrence of pericarditis masked by delirium, has recently been under my observation at the Louisville Marine Hospital. In this instance, much to my surprise, considering the gravity of the symptoms, the patient emerged

from the delirium, and, at the present moment, is convalescent, with, as I suppose, a heart permanently damaged. An account of this case I propose to give, with as much detail as shall seem advisable, taking the facts from the daily records at the bed-side.

John Maher, aged 24, Irishman, laborer, admitted October 24, 1853. On attempting to obtain the previous history of the case, Dr. Dickinson, resident physician at the Hospital, found him too dull to give any connected account either of past or present symptoms. So far as he could gather any information from his disconnected replies to questions, he thought his disease was intermittent fever, and directed twenty grains of the sulphate of quinia to be given in divided doses during the following twenty-four hours.

At 8 or 9 o'clock, P. M., he had chill and rigors, followed by febrile movement and sweating.

On the 25th he appeared delirious, frequently getting out of bed, and appearing to be bewildered. He talked but little. During the night of this day, he got up several times without any apparent object, and was taken back to the bed by the ward attendant. Once, after getting out of bed, he fell to the floor apparently from weakness.

On the morning of the 26th, he was found to be in a state of unconsciousness. On this morning, for the first time, my attention was directed to the case. The description recorded by me at the bedside was as follows:—The patient has lost one eye. The other eye remains open. He takes no observation of persons or things around him. The pupil is dilated. He winks frequently, and always when the finger is brought into close proximity to the eye. He maintains the dorsal position. Remains taciturn, and cannot be roused to reply to questions, or take notice of anything. The skin is bathed in perspiration, sweat standing in drops on the face. Urine has been emitted freely in bed. The salivary fluid collects and escapes at the angles of the mouth. He does not swallow when drink is placed in the mouth. Respiration thirty-six per minute, and somewhat labored, but rhythm normal. Pulse one-hundred-and-eight, and moderately full.

The impulse of the heart extends over an area of from two to three inches in diameter; the lower boundary being about half an inch below the nipple. The nipple is nearly the centre of the

area of impulse. The impulse is forcible. There is dullness on percussion, below the lowest point of impulse, and over an abnormal area in the precordia, the precise extent not determined. The heart sounds are unattended by *bruit* of any kind.

A blister, six by six, was directed to be applied over the precordia.

The foregoing is all that was recorded at this period in the history of the case, relative to physical signs. The examination was not so complete as, afterward, I could have desired. With reference to physical explorations, too, I had occasion, for some time afterward, to regret that the precordia had been vesicated, inasmuch as they were precluded by the condition of the surface. I may add that at this time, and for some time afterward, I supposed that the nature of the affection would soon be determined by an autopsy. The death of the patient was daily expected. To the reader practically acquainted with the subject of physical diagnosis in its relations to pericarditis, it is submitted how far the physical signs already stated indicate the existence of that affection. The demonstrative physical proof of its existence, viz: a friction sound, was not discovered. The physical signs existing at a subsequent date will be presently stated.

Oct. 27.—The patient remained through the afternoon of the 26th inst., in the same condition as that described in the morning record; but at nine in the evening, he had a partial return of consciousness. He was tolerably quiet during the night, occasionally changing his position. This morning he has been very restless, tossing about, and throwing himself from the bed, so that it became necessary to transfer him to a bed on the floor. He frequently groans and mutters. He has taken neither food nor drink since yesterday. Fluids introduced repeatedly into the mouth were neither swallowed, nor voluntarily ejected, but remained in the mouth until they ran out. No dejection, but urine passed freely in bed. At the time of the daily record he lay with his eye open, frequently winking, the pupil dilated. The eye did not close on approximation of the finger till the eye-lashes were touched. The respiration was labored, the expiration somewhat prolonged. The skin was cold. Pulse scarcely appreciable.

The blister on precordia produced good vesication.

Oct. 28.—Patient was quiet during the night. He has taken no drink or nourishment since the 25th inst. He has lately resisted the introduction of fluids into the mouth by closing firmly the teeth. On making efforts to cause him to swallow, at the time of the examination, he resisted, but he swallowed a portion of the liquid introduced into the mouth. The skin on this morning was warm; the pulse more developed than on yesterday, and eighty per minute. Respirations twenty-eight, and somewhat labored; inspiration abnormally intense, with dilation of *alæ nasi*. Occasional moaning with expiration. No dejection. Has urinated freely in bed.

Spirit and water, and beef essence prescribed, to be given as freely as practicable.

Oct. 29.—Marked improvement, the patient lying tranquil, eye opened, taking some notice of persons and things around him. Respiration normal. Skin warm and mellow. Pulse tolerably developed, soft, and not accelerated. During the latter part of preceding night he talked much, and loudly, frequently shouting. Took food this morning, viz tea and bread. Had a dejection, and urinated freely—both in bed. Was not rational. Did not reply coherently to questions. This peculiarity was noticed, viz. he directed his vision to some point, now a portion of the pillow, and now his hand, protruded his tongue toward it, and then slowly grasped it with his lips and teeth. This he repeated several times during the examination.

The precordia was quite sore from the blister, preventing physical exploration. At the inferior portion of chest, posteriorly, the percussion sound was clear.

Treatment had consisted of diffusible stimulus, carbonate of ammonia, and the essence of beef. The spirit and ammonia were suspended on this day, and nutritious diet continued.

Oct. 30.—Through the previous day and night, the patient, most of the time, was restless, throwing himself about, getting up, and calling names of different persons. On this morning his expression was idiotic. His eye was open, and he looked about with a vacant stare. He resisted, moderately, attempts at a physical exploration. Twice he said, while the record of symptoms was being made, "I beg pardon." These words were uttered sponta-

neously, with great slowness and hesitancy. He did not reply to questions, and was taciturn the greater part of the time. He did not utter any connected sentences. Protruded his tongue very slowly when requested. No dejection, but free urination in bed. Took food and drink when presented, eating very slowly, and looking at the attendant with an expression devoid of intelligence.

The blistered surface in precordial region prevented a satisfactory physical exploration. The action of the heart, denoted by the sounds, was irregular. A point of impulse is neither seen nor felt, but, owing to soreness from the blister, palpation could be employed but imperfectly.

Clearness of resonance on percussion existed at the lower part of the chest, posteriorly, on both sides.

Marked tenderness on pressure (irrespective of the blister) existed at the lower part of the chest in front.

Skin cool. Pulse forty eight, and quite feeble. Respiration thirteen.

Diffusible stimulus, in small quantities, was directed, with nutritious diet. No other treatment.

Oct. 31.—Had an attack of convulsions in the night time, and another at 10, A. M. Pulse sixty six. Respiration sixteen. State of intellect not materially altered. Could not be made to protrude the tongue.

Nov. 1.—Attacks of convulsions recurred three times during the preceding day, and once in the night. During these attacks the upper and lower extremities, and the muscles of the face, were convulsed with considerable violence. They were about an hour in duration. There was no foaming at the mouth, nor did the attendants notice any marked disturbance of the respiration. Urine passed in bed. No dejection. He frequently got out of bed, and appeared as if he fancied persons were in pursuit of him intending violence. He endeavored to break the walls of the room, and his hands were accordingly placed in a restraining apparatus. Protruded his tongue when requested, and kept it protruded till told to withdraw it.

Talked and shouted incoherent words much of the night.

Respirations not accelerated, and normal in rhythm. Pulse seventy-eight, and feeble. Resisted efforts to examine the precordia.

Nov. 2.—Restless night, rolling about the room, and shouting. Copious dejection in bed. Asks for nothing, but takes food and drink readily when presented. Pulse ninety, and quite feeble. Skin cool. Respiration twenty.

Nov. 4.—On the 2d inst. he continued wakeful, shouting, with occasional manifestations of hilarity. But on this day he became quiet, and slept most of the time. Took food and drink readily, and asked for bread. Did not reply to questions generally, but, in answer to how he felt, said he felt well. Protruded tongue after repeated directions to do so, as before, very slowly. Pulse one-hundred-and-eight. Skin warm and moist.

The blistered surface was nearly well, but tenderness on pressure existed, not only over the sore surface, but in the neighborhood.

Nov. 6.—Patient rational, replying to questions readily. Said he felt well, except that he had some pain in the left breast above the nipple, sharp, and felt on taking a full inspiration.

Nov. 7.—*Physical Signs.*—Over precordia transversely through the line of nipple, flatness on percussion as far as the nipple. Vertically, dullness distinct, but not marked, over second rib, flatness from third rib.

Impulse extremely feeble, being just appreciable between fourth and fifth ribs, about half an inch below, and the same distance to the right of a vertical line passing through the nipple.

Nov. 8.—Reported feeling quite well, without pain, but had had pain the previous night in left side over the third rib. Placed his hand in that situation when requested to indicate the seat of pain. Said that the pain was short, and particularly acute when he coughed. Had no recollection of the events of the previous fortnight. Stated that he was ill for two days before he came to the Hospital, and that he suffered chiefly from pain in the left breast.

Physical Signs.—Percussion-sound clear over both sides, exclusive of precordia, and the respiratory sounds on both sides healthy.

The heart impulse between the fourth and fifth ribs, half an inch below the level of the nipple, and the same distance to the right. Impulse at the examination distinctly seen and felt. The point of impulse not materially changed by assuming the sitting

posture. Action of heart quickened by this change of position. Notable dullness, amounting nearly to flatness, below third rib. Above third rib percussion-sound clear.

Flatness transversely from sternum of nipple, and dullness to the left of nipple. Breathing movements on both sides presented no disparity.

No heaving impulse in precordia. Sounds of heart normal.

Diarrhœa was somewhat troublesome at the date of this record.

Nov. 15.—Improvement in intellect and strength had continued. He was now up and dressed, and able to walk about the ward.

Physical Signs.—A movement seen in the precordial region, extending from the third to the fifth ribs. The visible movement not uniform at different points of the space over which it was apparent. There appeared to be an impulse between fourth and fifth ribs, and simultaneously, a visible depression (not impulse) between the third and fourth ribs, and just below the fifth rib. By palpation no impulse was appreciable, except between fourth and fifth ribs, nearly on a level with the nipple, and half an inch to the right. In this situation, the impulse was marked but not strong. Above, between third and fourth ribs, and below, between fifth and sixth ribs, there was no appreciable impulse, but evidently a *drawing in*, with the action of the heart, of the intercostal spaces.

On percussion, flatness existed transversely to about half an inch to right of nipple, three and a half inches from median line, and marked dullness to about an inch to the left of the nipple. Vertically, on line falling about an inch to the right of nipple, marked dullness over third rib, and flatness over fourth rib, and extending to lower part of chest. No friction, or any abnormal sounds discovered on auscultation.

At the time I am writing this report, Nov. 25, the patient is still in the Hospital. He complains of nothing but weakness. His appetite is good, and bowels regular.

I have given the history of this case with a minuteness which may, to some, seem tedious, in order, *first*, that the reader may be able to judge of the correctness of the diagnosis. The interest and importance belonging to the case, depend, of course, on there being sufficient evidence of the existence of pericarditis. The

points on which the diagnosis are based, to recapitulate briefly, are as follows:—Increased area of dullness in precordia, together with abnormal degree of dullness, or flatness; elevation and diffusion of the impulse; irregularity and feebleness of the heart's contractions, and, at the time of convalescence, in connection with persistence of the elevated impulse, depression in the intercostal spaces with the heart's action. I am free to admit that the results of more complete exploration during the progress of the affection would be desirable. But the condition of the precordia, and the state of the patient, rendered it difficult to make satisfactory examinations at that time. Farther efforts for this object, however, would have been made, had it seemed probable that the case would have terminated so favorably as it has done. In connection with the physical signs denoting pericarditis, it is to be considered that acute pulmonary disease was excluded by the absence of marked signs, as well as symptoms pertaining to the lungs. The symptoms (in distinction from signs) pointing to pericarditis were pain in precordia, which was the prominent symptom before the patient entered the Hospital, and was experienced after his consciousness returned, together with tenderness in the same region, evidently not dependent on the blister; and, during the severity of the disease, notable oppression or embarrassment of the circulation.

Assuming the diagnosis to be correct, a *second* object of the report is to describe fully the cerebral symptoms, which were the prominent features in the case, and were well calculated to mask the cardiac affection. If the reader will take the trouble to refer to the case previously reported by me, he will find a striking resemblance in the character of the delirium in that case, to that of the case now reported; and in both the distinctive traits are those observed in the cases referred to by Drs. Watson and Burrows. The peculiarities are so striking, that, after having observed a case, the practitioner is afterward led, at once, therefrom to recognize the disease. The characters of the delirium are, stupor and wakefulness combined, the patient lying with the eyes opened, and fixed in one direction, not replying to questions, and insusceptible of being roused by any exertions for that end; this state followed by paroxysms of active delirium, the patient shouting,

and apparently laboring under the fear of harm, with occasional manifestations of hilarity.

A striking point of resemblance in the present case, to that of Crotty, formerly reported, is that each appeared to imagine he had committed some offence for which he was to be punished. In the case of Crotty, this was shown by his uttering, at one time, in reply to questions of any kind addressed to him, the word "guilty," and afterward asking why he was not hanged. In the case of Maher, among the first words spoken were, "I beg pardon," which were repeated several times, and nothing else said.

The delirium which characterized both cases was entirely different, so far as my observations go, from that belonging to other affections attended by mental aberration. It was not like the delirium of delirium tremens, of continued fever, or of encephalitis; and, if the peculiarities which distinguished these two cases exemplify an uniform kind of delirium occurring in certain cases of acute pericarditis, its character is so unique and striking, that it should not only suggest the disease, but is entitled to be considered presumptive evidence of its existence.

In view of the consideration just stated, I trust that I shall not be thought to have devoted, in this article, too much space to the report of a single case, more especially when it is considered that similar cases are rarely met with.

Art. II.—A CASE OF INVERSION OF THE UTERUS.

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Mrs. Yengst, a German woman, aged forty years, the mother of seven children, all natural births, was taken with the pains of her eighth labor at 9 o'clock, A. M., November 29th, a German midwife in attendance; the labor gradually progressed and was successfully completed, by the birth of a healthy living child, at six o'clock, A. M., November 30th. The placenta and membranes with an umbilical cord twenty-two inches long were expelled very soon after the child; the membranes spontaneously gave way just before delivery, the amount of amniotic liquor not noticed. The labor though rather tedious was not productive of any very marked exhaustion, and the patient was represented to have borne up under the effort, with strength and firmness; the only circumstances worthy of note were moderate hemorrhage throughout its entire progress, coagula of blood passing from time to time, and the coiling of the funis thrice around the child's neck; the hemorrhage was not sufficient to produce material exhaustion, but excited some apprehension on the part of the midwife.

The patient was delivered in the recumbent position, on her back, and the child was neither suddenly nor violently expelled. The placenta was discharged without any assistance from the midwife. Profuse hemorrhage followed the delivery of the placenta, succeeded by faintness and great prostration; the midwife ignorant of the nature of the difficulty and unprepared and unwilling to act, immediately dispatched the husband for me. I saw her at three-quarters past six o'clock, three-quarters of an hour after the completion of the labor. Her condition was then one of extreme prostration; blood had been flowing from her, "like water from a pump," to use her own language; her face was blanched and anxious; hands and feet cold; pulse rapid and

feeble; respiration disturbed, with a sense of a want of air; the bed was flooded with blood, though the midwife had been occupied in removing a portion of it to a tub which stood near at hand.

Having administered brandy very freely, as immediate death seemed to be impending, I proceeded to inquire into the nature of the difficulty. Upon placing the hand upon the hypogastrium, instead of finding the uterus hard, globular and contracted as it should be, it passed deep into the cavity of the pelvis and discovered the fundus depressed, and presenting a cup-like form. This state of things at once suggested inversion of the womb, as the probable difficulty, a suggestion confirmed by finding a large, rough, bleeding mass filling up the vagina and protruding slightly beyond the vulva. The diagnosis being obvious, the patient somewhat revived by the brandy, and appreciating the importance of prompt action, I attempted, at once, the reposition of the organ. This was easily accomplished, by embracing the inverted fundus between the thumb and fingers of the right hand, the most dependent portion resting upon the palm, and by gently and gradually pushing it up before them; it was returned with unexpected facility, the os uteri offering scarcely an appreciable resistance. The hand was carried into the cavity of the uterus and retained there sufficiently long to receive assurance of a complete restoration of the organ, and to recognize very decided contraction. Sixty drops of tincture of opium were administered in warm brandy and water, and I left the patient, very shortly after, in a satisfactory condition.

During the day of the 30th, and the following night, Mrs. Y. suffered slightly from after-pains, accompanied by moderate flowing; in the afternoon, nervous and vascular reaction ensued and ran quite high; she complained very much of palpitation of the heart. The after-pains and reaction were treated by opiates; under these and a properly regulated regimen, she gradually improved, presenting scarcely a symptom of uterine inflammation, and was discharged convalescent on the 8th of December. At the present date, 18th December, she has recovered sufficiently to walk about, and to resume her domestic duties, to a limited extent.

This case of inversion of the uterus is reported, not because it

presents any point of special interest or peculiarity, in connection with this rare form of accident, but as a contribution to the statistics of the subject, and as the basis of a few remarks in regard to its causes, symptoms, and treatment.

A very relaxed and uncontracted condition of the uterus is universally recognized as essential to the production of inversion; this state of the organ may be esteemed a requisite predisposing cause, whilst a considerable diversity of agencies may act as exciting causes.

Undue traction of the funis while the placenta is attached to the uterus is one of the most frequently assigned causes of inversion; when the accident occurs, improper management in this respect, on the part of the accoucheur is apt to be suspected. This cause is fully sufficient for the effect in a relaxed condition of the womb. The mere weight of a large and attached placenta, in a similar state of the organ, may also produce the effect, without traction of the funis. This may occur, even in the recumbent position of the patient, but is more likely to take place when she is somewhat elevated in the bed, or when she is delivered in a sitting position, as is common in some parts of the country.

It is not necessary that the placenta be attached to the uterus, in order that traction of the cord may produce inversion. When detached and pressing upon the os uteri, or detained in the vagina, it may excite, by reflex action, the abdominal muscles and diaphragm, to powerful contractions, sufficient not only to extrude the placenta, but also to force down the fundus of the uterus, the lower portion of the organ being, to some extent, a fixed point. This may occur, whether the funis be pulled upon or not, the mere pressure of a foreign body in the uterus and vagina being sufficient to excite the involuntary contractions of the abdominal muscles and diaphragm.

The introduction of the hand into the vagina and cavity of the uterus to search for the placenta will often suffice to produce a straining effort, on the part of the patient, adequate to force down a relaxed fundus. It is even possible that the impression left by the contact of the child with the nerves of the vagina may be reflected, for some length of time after the birth of the child, and sustain the action of the diaphragm and abdominal muscles.

Inversion has followed rude efforts to detach an adherent placenta, by the hand in the uterus; cases of this kind are reported. It is very possible that a similar result might be produced by the common practice of pressing and kneading the uterus, through the walls of the abdomen, for the purpose of soliciting prompt uterine contractions and the speedy expulsion of the placenta, in cases of inertia following delivery. Strong downward pressure directed upon the fundus of a very relaxed uterus would be fully adequate to cause such a dislocation, and the mere probability of such an effect suggests a caution as to this very prevalent mode of practice.

Assuming the erect position has been known to cause inversion, even many days after labor, and the accident has followed the effort connected with alvine evacuation.

A short funis has been assigned as a cause of inversion. A short funis with the placenta attached to the uterus in a state of relaxation would be sufficient to produce inversion, and is in effect the same as intentional traction of a long cord.

Violent expulsion of the child by which it is thrown far from the mother, and the cord thus made forcibly to drag the placenta, either attached or unattached to the uterus, is equivalent to a short cord in its influence upon the uterus, or to undue traction of a cord of the ordinary length. The same is true in reference to the coiling of the cord several times around the neck of the child, a condition which existed in the case of Mrs. Y.

A large quantity of amniotic liquor has been mentioned as contributing to inversion. When a large amount of this fluid is discharged from the uterus simultaneously with the child, the organ is left in a state indisposed to contraction; it is surprised into a condition of inertia. In cases of this kind, profuse homorrhage is very apt to ensue. A physical and vital state of the organ certainly exists here strongly predisposing to the facile action of the other causes assigned; apart from these, it is not easy to comprehend how a large quantity of amniotic water can act as a cause of inversion.

According to the statistics of inversion of the uterus, published by Dr. S. B. Hunt, in the November number of the Buffalo Medical Journal, "the liability to inversion decreases, but not to any

marked degree, with the number of children which the woman has borne." This result of Dr. HUNT's analysis is somewhat unexpected, and would scarcely have been anticipated, reasoning from the supposed predisposing cause of inversion. A relaxed and uncontracted state of the uterus seems to be an essential prerequisite to the efficient action of the immediate and determining causes, such as traction of the funis, a short funis, the erect position, abdominal pressure, &c.; it seems almost impossible for the body and fundus of the uterus to be thus displaced, when firmly contracted. Dr. WM. HUNTER, quoted by Dr. HUNT, remarks, "when a uterus is flaccid it is inverted as easily as the finger of a glove, but when it is hard, globular and contracted, it is as difficult to invert as a jack-boot." The question, apart from the testimony of statistics, resolves itself into the further question, whether this necessary flaccidity of the uterus is more apt to supervene after first labors, than in women who have had several children. General experience, which probably cannot be relied on so fully as the results of statistical analysis, would decide in favor of its more likely existence under the latter circumstances. After-pains, due to imperfect contraction of the womb, are very uncommon, with first labors, and become progressively more common, as a general rule, with each succeeding labor. Profuse *post partum* hemorrhage is more prone to occur in a woman who has had many children than after the birth of a first child, for the reason that contraction is not so prompt and vigorous. An analysis of a larger number of cases of inversion would possibly lead to a result somewhat different from that rendered by the interesting researches of Dr. HUNT. First labors are often very protracted, and patients frequently emerge from them very much exhausted; both general and uterine exhaustion may exist, a condition strongly predisposing to the easy operation of those causes supposed to be adequate to induce inversion. If it be true that this accident is more frequent after first and few labors, than in those who have borne many children, this may be the explanation.

Death may ensue very suddenly upon inversion; it may occur before reposition of the organ, though this be promptly attempted, and sometimes very shortly after; in either case it may result from nervous shock or exhaustion, indicated by syncope without

hemorrhage, by convulsions without hemorrhage, or there may be profuse hemorrhage superadding its depressing influence to the shock. Hemorrhage alone may be the cause of death, but writers insist, probably too exclusively, upon it as the urgent and immediately dangerous symptom of inversion; it is an important and alarming symptom, but the statistics of Dr. Hunt go to prove that "hemorrhage does not occur to any more fatal extent than does convulsion or syncope." Hemorrhage at once arrests and directs the attention of the intelligent practitioner to the condition of the uterus; examination detects the cause and leads to a prompt interposition of skill. Convulsion and syncope, from the influence of authority, are not so apt to suggest a suspicion of inversion, and thus lead to a prompt examination. In this way, it is possible that many of the chronic cases of inversion are brought about, the true nature of such cases not being detected in time for reposition. It is even possible that life has been lost in many instances, without the cause of death being ascertained. Syncope and convulsions, after labor, though unattended by hemorrhage, may often depend upon unsuspected partial or complete inversion.

Inversion of the uterus demands very immediate attention, not only on account of the threatening symptoms connected with it, but also from the fact that delay increases the difficulty of reposition, in proportion to its duration. All writers insist most emphatically upon the necessity of early attempts at restoration, and such necessity must be obvious. In a very short time, the os uteri contracts and girds the inverted portion so firmly that it cannot be moved. Time is considered so important an element in the successful management of this difficulty that it requires to be measured by minutes; the lapse of an hour may lead to a failure under the most skillful application of the art. Attempts to replace the organ after it has been inverted for several hours and even for many days, have not, however, always been fruitless. Dr. Hunt reports, upon reliable authority, success of the operation, after the expiration of twelve weeks, and at various periods earlier than this. These fortunate cases should not, if possible, be permitted to retard or control the action of the practitioner.

Notwithstanding the necessity which seems almost imperative for early efforts at reposition, regard should be had to the condi-

tion of the patient. The nervous shock or the hemorrhage may be such as to render it hazardous to subject the patient, at once, to the further depression necessarily incident to the operation of reposition. If the prostration be very great, some degree of reaction should be established, in the first place, by the liberal use of diffusible stimulants. The same rule of practice, in this regard, which guides the practitioner in the treatment of a case of placenta prævia with profuse bleeding and great prostration, should guide him in a case of inversion. No prudent practitioner would attempt delivery by version of the child, in such a case, until he had rallied his patient by the administration of brandy and laudanum in quantity adapted to the emergency. Though it be dangerous to wait, it is more dangerous to go on, under such circumstances, until some reaction has been restored by artificial stimulation. Surgical operations performed upon patients who have received severe injuries and great shock, immediately after the accident, often determine a fatal result, which might have been averted by a little delay.

While excessive prostration calls for some delay in the operation of reduction, a certain amount of it, with the accompanying relaxation, will materially facilitate success. In the case of Mrs. Y. the ease with which relief was afforded, was doubtless due, in some measure, to the relaxation caused by excessive loss of blood.

According to the analysis of Dr. Hunt, "the placenta is adherent in a large proportion of cases." The question as to the practice proper to be pursued, when this is the case, is an interesting and important one. An attached placenta materially embarrasses the reduction, while its detachment may lead to serious and fatal hemorrhage. On the other hand, if the placenta be returned, its subsequent detachment in the cavity of the uterus may also be attended by very free hemorrhage, and if not very carefully done, may again induce inversion. This latter result has occasionally occurred, and is mentioned among the causes of inversion. It rarely happens that when the placenta is adherent, it is entirely so; its precipitation must necessarily sunder some of its connections; when this is the fact, its complete separation and removal will facilitate and expedite the reduction to such an extent as fully to compensate for the increase of the bleeding surface. Though a

larger quantity of blood would certainly be lost from the whole placental surface of the womb than from a portion, in a given period of time, less time and less effort would be required to effect reduction with the placenta detached, and the amound of hemorrhage proportionably diminished. Practitioners of high authority differ in opinion upon this matter. Dewees, Gooch, Burns, Denman and Blundell, very emphatically advise the reduction, if possible, without previously detaching the placenta; Baudeloque, Churchill, Boivin, and Moreau, pursue the other practice. Dr. Ashwell thinks that the danger of hemorrhage is slight in those cases in which the contraction of the cervix hinders the easy reduction of the entire mass; the same contraction ligates the blood-vessels and prevents the loss of much blood. He concludes that "where the protruded organ and its attached placenta do not together make up a large tumor, and where the os does not constrict its upper part, reduction of the whole may be at once attempted; where, however, these favorable conditions do not exist, it will be wiser to ensure reposition by previous separation." The analysis of Dr. Hunt tends to the conclusion that the danger of hemorrhage has been much exaggerated. This gentleman favors separation as an important antecedent step of the operation. His experience and analysis of sixty-seven cases justify the following practical inferences on this point:—that the placenta "when adherent should be removed prior to any attempt to reduce the inversion; such removal of the placenta does not increase, but rather decreases the risk of hemorrhage, while it facilitates reduction; the returning of the placenta, in reducing the inversion, complicates the after treatment, and adds to the danger of hemorrhage, while it retards and renders difficult the reduction."

The case of Mrs. Yengst was unattended by any inflammation of the uterus. The organ was so speedily returned, and subjected to so little manipulation, that this is not remarkable. Cases of inversion seem, however, to enjoy a singular immunity from metritis, even when the reduction has been difficult; this is so far true that Dr. Hunt's analysis leads to the deduction that "there is little danger of metritis occurring after inversion."

Art. III.—UNVERSITY CLINIQUE—CASES AND OBSERVATIONS IN SURGERY.

BY S. D. GROSS, M. D., PROFESSOR OF SURGERY IN THE UNIVERSITY OF LOUISVILLE.

Case 1.—*Strumous Ophthalmia.*

The patient, a girl, aged ten years, always enjoyed good health until about three years ago, when she began to suffer from inflammation of the eyes, from which she has never been entirely free since. The prominent symptoms, all along, have been intolerance of light, larcrymation, and adhesion of the edges of the lids in the morning. There has been but little discoloration, at any time, so far as we can ascertain, of the eye; and the conjunctiva, this morning, is nearly of the natural aspect, except that there is here and there a straggling vessel, extending inwards towards the cornea, and carrying an undue quantity of blood, thus giving the membrane a slightly congested appearance. Each cornea retains its natural transparency, except at one or two small points, where there is some degree of haziness, dependent upon the presence of plastic matter. The opacity is quite superficial, and apparently of recent formation. The conjunctival lining of the lids is abnormally red, but in a less degree than in ordinary inflammation. It is free from granulations and hypertrophy. The edges of the lids are somewhat discolored and inflamed, and the Meibomian follicles are, perhaps, a little too much developed. No pus or mucus is seen upon the eye, or the lids, but along the eyelashes are a few thin, delicate, bran-like scales, so common in this form of inflammation. The patient, it will be noticed, is unable to throw her head up, so as to face the light; on the contrary, she screens the eyes with her hands, to prevent the light from falling upon the retinæ.

Such are the appearances and condition of the affected organs.

If you will now look at the patient, you will observe that she is in delicate health, and that she has light hair, blue eyes, with long lashes, a fair complexion, tumid lips, and a protuberant abdomen. The extremities are habitually cold, owing to the languid state of the circulation; the pulse is feeble and rather slow for one of her age, and there is a slight enlargement of several of the lymphatic ganglions on the right side of the neck, along the inner margin of the sterno-mastoid muscle. The bowels are regular, and the appetite and sleep good. I cannot ascertain that the mind is particularly sprightly. The child has evidently not had the advantages of education, which has been interrupted by the state of her eyes. Her parents are both living, and they appear to be free from scrofulous disease.

The *symptoms*, in this case, are, it will be perceived, of a mild character. In your practice, you will often meet with the disease in a much more severe and distressing form, attended with the greatest possible intolerance of light, excessive lacrymation, and violent pain. I constantly see cases in which the smallest ray of light is productive of the keenest suffering, and where, consequently, the patient uses every possible precaution to prevent its intrusion. For this purpose he generally, if he be a child, as is commonly the case, creeps into the darkest corner of his chamber, where he covers his eyes with his hands, or buries his head in a pillow, or, perhaps, in the lap of his mother. In this condition he often remains for hours, afraid to change his posture, lest the light should meet his eyes, and thus increase his distress. Children thus affected frequently experience an aggravation of all their suffering even from the light of the moon and of the stars, such is the excessive sensibility of the retina. Photophobia, then, or intolerance of light, is a most important diagnostic symptom in this affection, and one which no practitioner should disregard.

The lacrymation also exists in various degrees, under different circumstances, and in different individuals. In most of the cases that have fallen under my observation, it has been, at one stage or other of the complaint, a prominent symptom. Exposure to light and cold always increases it. The tears are usually hot and scalding, and their discharge is almost always attended with temporary relief.

Sometimes they are so acrid as to irritate the cheeks, causing them to becomered and swollen. The quantity of lacrymal fluid that is thus evacuated in the twenty-four hours may amount to several drachms.

It is rare, in strumous ophthalmia, to witness a copious discharge of mucus, or of muco-purulent fluid. Even when there are excessive photophobia and great lacrymation, it is seldom that there is much secretion of this description; often, indeed, not enough to agglutinate the edges of the lids. In this respect, scrofulous inflammation forms a striking contrast with some of the other varieties of ophthalmia, in which an immense quantity of mucus, or of mucus and pus, is discharged during the height of the morbid action, and even during its declension.

We have seen, in the patient before us, that there is but little redness in the conjunctiva. This is another important symptom in this variety of ophthalmic disease. In ordinary inflammation, discoloration of this membrane is a constant occurrence, and, generally, it is so conspicuous as to attract, at once, the attention both of the physician and of his friends. In strumous inflammation the vessels observe a straggling arrangement; they are seldom very turgid, and they extend from the circumference of the ball inward towards the cornea, where they are often congregated into little groups, or clusters, beautifully interlacing with each other. When the disease is violent, or of long-standing, the vessels occasionally pass over the cornea, either singly or in parallel lines, separated by narrow intervals. In ordinary ophthalmia the vessels are extremely numerous, and lose, so to speak, their individuality. In a word, there are hundreds where there is one in strumous ophthalmia.

Another important symptom in this form of ophthalmia is the existence of little minute vesicles at the margin of the cornea, occurring either separately or in groups, and varying in size from the smallest perceptible speck to that of an ordinary pin's head. They contain each a minute quantity of serum, and they are frequently encircled by a delicate plexus of vessels, which impart to them a very beautiful and interesting appearance. Their shape is globular, ovoidal, or angular. Sometimes they exist partly on the sclerotica and partly on the cornea. They are witnessed in

no other form of ophthalmia, and hence they are of great value as a diagnostic sign.

Strumous ophthalmia seldom continues long without giving rise to opacity of the cornea. This effect, like some of the others that have been mentioned, presents itself in different degress, from the slightest haziness of the part to complete opacity. In the latter case, it is always to be greatly dreaded, inasmuch as it is generally followed by total blindness. Its occurrence should always, if possible, be prevented, not only because it is liable to impair the sight, but because it must necessarily, when irremovable, disfigure the eye and injure the expression of the countenance. It need hardly be added that the immediate cause of this phenomenon is a deposit of lymph into the structures of the cornea.

Ulceration of the cornea is another effect of this variety of ophthalmia, and one, in fact, of frequent occurrence. It often begins at an early stage of the disease, and may proceed, with more or less rapidity, until it extends through the entire thickness of the membrane. The most common form of the ulcer is that of a dimple-shaped depression, with smooth and rather sharp edges, the surface looking as if a piece had been scooped out of it. Generally, the ulcer has a hazy appearance, but not unfrequently it does not differ in its color from that of the adjacent parts, and hence, unless the cornea is examined with great care, while the light is falling upon it at a particular angle, the disease may readily escape detection. Sometimes several such ulcers exist upon the eye, forming either simultaneously, or in pretty rapid succession. If permitted to progress, they occasionally extend through the different layers of the cornea, as far as the anterior chamber of the eye, the humor of which may perhaps escape through the abnormal opening, or, what is more common, the opening is closed up by the membrane of the aqueous humor, or even by the iris.

The pain attendant upon strumous ophthalmia is sometimes intense, while at other times it is very insignificant, if not wholly absent. In confirmed cases, it is always aggravated by the slightest exposure of the affected organ to the light, by medicated applications, by disorder of the bowels, by indulgence in eating, by rough contact, and by various other circumstances unnecessary to be mentioned. Occasionally it is situated deeply in the ball of

the eye, in the orbit, or at the base of the anterior lobes of the brain; sometimes it affects merely the lids and brows; occasionally it is most severe in the temple, forehead, or cheek. It may be sharp, shooting, or darting; dull, heavy, or aching; throbbing, or pulsatile; continued, or intermittent. Not unfrequently it assumes a neuralgic character, recurring periodically, like neuralgic pain in other parts of the body. Whatever may be its character, it is often so severe as to deprive the patient of sleep and appetite, and, indeed, of all comfort, for days and weeks together.

Strumous ophthalmia is rarely attended with any tumefaction of the lids. On the contrary, these structures usually retain their normal shape and size; but, in consequence of the excessive intolerance of light, they often present a remarkably drooping appearance, owing to the manner in which they are drawn over the eyes. When the disease is very protracted, the edges of the lids frequently become inverted, so that the ciliæ impinge constantly against the cornea, thereby inducing opacity of this membrane, great increase of pain, and additional inflammation. Although, in general, there is an absence of swelling of the lids, yet you will occasionally find this symptom to exist in a very marked degree. This is especially apt to happen in young children of a leucophlegmatic habit, with a thick upper lip, a tumid belly, and a soft flabby tongue, along with great derangement of the digestive apparatus. The whole system, in such cases, seems to be surcharged with strumous disease, which, in consequence, it is extremely difficult to dislodge from the eyes, which frequently become, so to speak, its victims.

In many cases there is an appearance of little vesicles on the cheeks, the inferior lids, round the nose, or on the lips. Their number varies from two or three to several dozens; their volume rarely exceeds that of the head of a small pin; and their contents are of a serous character. They have a whitish, almost pearly, aspect, are usually discreet, though often closely grouped together, and rest, as it were, upon a reddish base. These vesicles, according to my observation, are most common in children of a deeply-marked strumous habit, and they seldom manifest themselves until after the inflammation has made considerable progress.

I always look upon them with a feeling of misgiving in regard to the ultimate issue of the case; for their presence almost invariably denotes great obstinacy in the morbid action, and proportionate difficulty in effecting a prompt and permanent cure.

From the symptoms which have now been detailed to you, and from the appearance of the patient before you, you can hardly fail in establishing the *diagnosis* of this affection in any case that may fall under your observation. The excessive intolerance of light, the unwonted lacrymation, the absence of redness in the conjunctiva, together with the peculiar straggling arrangement of its vessels, the want of tumefaction, and the manner in which the lids are drawn over the ball of the eye, are signs which, once observed, can never be mistaken. Add to these phenomena the fact that the disease usually arises insiduously and without any assignable cause; the strumous appearance of the features; the coldness of the extremities; the tumid condition of the belly; the formation of vesicles on the face, and various other evidences of the strumous diathesis, and all doubt respecting the true nature of the case must instantly vanish. Indeed, no practitioner, unless he is culpably ignorant of ophthalmic diseases, can possibly commit an error of this kind.

The *prognosis* in this disease must necessarily be influenced by various circumstances, as, for example, the progress and extent of the morbid action, the state of the patient's health, and the nature of our remedies. In the milder forms and earlier stages of the malady, and under proper management, recovery of the affected organ may generally be reasonably predicted. But, under opposite circumstances, the worst consequences may, not unfrequently, be looked for. Ulceration of the cornea often extends, despite our remedies, to a great depth, and sometimes even to complete perforation; an event which is sure to be followed by permanent impairment, if not total loss, of sight. Superficial opacity, even when it is diffused over the greater portion of the cornea, is generally readily amenable to treatment, but when it involves several of the layers of the membrane, or when a considerable period has elapsed since its formation, or, in other words, when time has been permitted for the organization of the lymph, upon the presence of which the opacity depends, then the

case will necessarily be unpromising, both as it respects the future appearance of the eye and the amount of vision. It is fortunate that strumous inflammation of the eye rarely terminates in gangrene of any of the structures of this organ. Such an event, judging from my own observation, is extremely infrequent.

Of the exciting *causes* of this disorder very little is known with any degree of certainty. Very frequently its origin is ascribed to circumstances which have no agency whatever in its production. Sometimes it is directly traceable to external injury, as a blow, or a wound; in many cases it is apparently brought on by long exposure of the eye to a strong light, or by excessive fatigue of the organ, induced by reading, writing, or sewing. Suppression of the cutaneous perspiration is probably another, if not a frequent, cause of the disease. In young girls, I have occasionally seen it connected with irregularity of the menses, but whether as a cause or an effect has not always been apparent. Perhaps the most common cause of all is derangement of the digestive apparatus. Whenever the predisposition exists, as it always does in this affection, almost any thing, however trivial, may bring on an attack.

The disease may be limited to one eye, or it may occur in both, either simultaneously or successively. I do not deem it necessary here to insist upon the minute, and, as I conceive, unmeaning divisions and subdivisions of strumous ophthalmia laid down by systematic writers on the diseases of the eyes. Such an arrangement can subserve no useful purpose in practice, and would be entirely out of place in a course of elementary teaching. It is sufficient to say that, in nearly every instance of this complaint, there is an involvement of the conjunctiva and cornea, if not also of the sclerotica and iris, and not simply of the conjunctiva and cornea, or of one of these structures alone, as one might suppose by reading the books, and neglecting observation. In all cases, the retina is either inflamed or morbidly sensitive, as is evinced by the excessive intolerance of light attending the malady.

The age at which this disease occurs is an important circumstance in its history. It is extremely rare for it to begin after the period of puberty, and in no instance have I witnessed its outbreak in middle or advanced life. It is emphatically a malady of infancy

and early childhood. According to my observation, it rarely shows itself before the age of eighteen months, or two years. It occurs in both sexes, and in every rank and condition of life, but more frequently among the poor, ill-fed, and ill-clothed, than among the refined and wealthy. The offspring of the consumptive, and of those who have suffered from tubercular disease of the spine, hip, arachnoid membrane, and lymphatic ganglions, are most liable to be affected.

From what I have said to you, on various occasions, concerning the *treatment* of this variety of ophthalmic disease, it is not necessary that I should, this morning, enter fully into the consideration of the subject. You will recollect that the great remedy is quinine, either alone, or in union with other means. I am very certain, from my experience in the management of this disease, that quinine deserves to be placed at the head of all other remedies in this variety of scrofulous affections, and yet, in making this remark, it is necessary to introduce a proviso, lest you should be thereby induced to invest it with a degree of confidence to which, valuable as it is, it is not entitled. What I wish to say is simply this, that this medicine will, if properly administered, that is, with due regard to the patient's system and other circumstances, produce the most prompt and salutary effects; while, if these precautions be neglected, it will either prove useless, or even cause mischief. There are, according to my experience, two distinct classes of strumous eye-cases. In the one, the patient is pale and thin, with a languid circulation, and cold extremities; in the other, he is stout and robust, the cutaneous circulation is active, and the hands and feet are habitually warm. Other points of dissimilarity readily suggest themselves, but these it is unnecessary to point out, as the ditinction which I wish to establish must be sufficiently apparent. Now, to treat such cases alike would be a palpable absurdity. It is only by properly discriminating between them that you can expect to arrive at a satisfactory result, as it respects the employment of this important therapeutic agent. Hence, one practitioner will often mismanage a case which another, having more judgment and more experience, will promptly cure, the disease, perhaps, disappearing as if by magic. Let us pursue this idea a little farther.

In the commencement of my treatment in both forms of the complaint, I usually prepare the system by the exhibition of a moderately brisk cathartic of calomel and rhubarb, to clear out the bowels and correct the secretions. If I have reason to suspect that there is much acid in the alimentary canal, I generally combine with the cathartic a few grains of bicarbonate of soda. Thus, a most effectual beginning is made in the treatment of the disease. If the case comes under the first division, that is, if the patient is pale and thin, and is habitually laboring under cold extremities, I now begin the use of quinine, seldom alone, but commonly in combination with sulphate of iron, tartar emetic, and opium, in quantities proportionate to the age and strength of the individual. For a child of ten years, the age of our patient, a grain and a half of quinine, one grain of iron, the twelfth of a grain of antimony, and the eighth of a grain of opium, carefully mixed, will be a suitable dose, repeated every eight hours, or, if the symptoms are urgent, every six hours, or four times in the day and night. If pills or powders are offensive to the patient, the articles may be given in solution, substituting laudanum or morphia for the opium. Where there is a highly-marked strumous diathesis, I sometimes use the iodide of iron instead of the sulphate, but in most instances I give the latter the preference. Tartar emetic I rarely omit in any case, from the fact that I regard it as one of the most valuable remedies we possess in the treatment of scrofulous disease, both of the eye and of other parts of the body. It is a powerful controller of capillary action, and at the same time a most potent sorbefacient: hence it is particularly applicable in all cases attended with deposits of coagulating lymph. The opium allays pain, renders the eye more tolerant of light, and prevents the antimony from irritating the stomach and bowels. The quinine and iron, whether in the form of sulphate or iodide, are powerful tonics; they improve and invigorate the digestive organs, increase the fibrin and coloring matter of the blood, equalize the circulation, and thus augment the temperature of the extremities, and powerfully aid in correcting the strumous diathesis. By means of these remedies, assisted by a proper diet and due attention to the bowels and secretions, almost any case of scrofulous ophthalmia

may, in the class of patients under consideration, be effectually relieved, and that, too, in a comparatively short period.

My favorite cathartic, in these cases, is calomel in combination with rhubarb; to which I occasionally add a few grains of soda, especially if there is reason to suspect the existence of a redundancy of acid in the alimentary canal. In a child from three to five years of age, about two and a half grains of the former, to five or six of the latter, should be given every fourth night. Occasionally the calomel may be advantageously replaced by blue mass; or, in infants, by the grey powder.

When the skin is dry and inactive, the tepid bath may occasionally be employed, or, what is better, the body may be sponged once a day with tepid salt-water, followed by frictions with a coarse dry towel. Flannel should be worn next the surface, both in summer and winter; and the greatest attention should be paid to the preservation of the temperature of the feet. Where they are habitually cold, they should be plunged, twice a day, for a few minutes at a time, into cold water, and then well rubbed with a dry cloth. It is a great mistake, in such cases, to bathe the feet in warm water, with a view to the restoration and maintenance of their temperature.

In the second class of cases, where the general health is apparently but little impaired, where the countenance is florid instead of being pallid, and where the extremities are, for the most part, warm, the quinine is most advantageously conjoined with sulphate of magnesia and tartar emetic, in the form of the saline and antimonial mixture. The following is the formula which I commonly employ under these circumstances:—

℞.		
	Quininæ sulph.	gr. xxv.
	Magnesiæ "	℥i.
	Antim. et potassa tartr.	gr. j½.
	Aquæ destillatæ.	℥ii.
	Syr. zingib.	℥i.
	Tinct. opii.	gtt. xxx.
	Acid. sulph. ar.	gtt. xxv.

M.

Of this mixture, which, considering its ingredients, may be re-

garded as an exceedingly elegant one, the dose is about one drachm for a child four or five years of age, repeated every four, five or six hours. If it induce vomiting, or nausea beyond a few minutes, it should be diminished. When the inflammation is very severe, I often omit the quinine until the disease has assumed a subacute character, and in that case also I occasionally take blood freely from the arm, or by leeches from the anterior part of the temples, within an inch from the outer commissure of the lids. In the strong and robust, iron, in every form, is totally inadmissible. The diet, too, must be more restricted, and more active purgatives are required. Indeed, the treatment should be strictly antiphlogistic, as much so as in inflammation of the eye from ordinary causes.

As to counter-irritation, collyria, and salves, so much used by practitioners, they cannot be too much or too pointedly condemned. Except in the latter stages of the complaint, in some rare circumstances, it is difficult to conceive of any case in which they would be likely to be beneficial. I am only speaking my real sentiments when I declare that I know of no class of remedies which have done more mischief, or which are so well calculated to fret and annoy the patient, and to support and perpetuate the morbid action. Setons are abominably filthy and painful, and should be discarded from this branch of surgery; tartar emetic ointment and croton oil cause injurious irritation; in short, the only eligible article of this class of remedies is a small blister behind the ear, or, what is preferable, because more easily managed and more permanent, a very small issue, in this situation, made with the Vienna-paste. This, when the eschar is detached, may be dressed, twice a day, with a little adhesive plaster, and will furnish a free discharge for several weeks, when, if necessary, it may be easily re-opened by the application of a little more paste, or some irritating ointment.

The best collyrium, undoubtedly, is a solution of nitrate of silver; but, to answer the purpose, it should be very weak, and not be used until the inflammatory action is greatly diminished, when it may assist in expediting and perfecting the cure by contracting the enlarged vessels of the conjunctiva and cornea, by allaying the morbid sensibility of the eye, and by promoting the absorp-

tion of effused lymph. The strength, at first, should rarely exceed half a grain to the ounce of water, which may be gradually increased to a grain, or even twice, thrice, or four times that quantity, according to the circumstances of the case. Sulphate of zinc, acetate of lead, Goulard's extract, and similar articles are worse than useless.

When there are ulcers on the cornea, and they do not yield to the remedies already enumerated, they should be touched, as lightly as possible, once every other day, with the point of a camel-hair pencil wet with a weak solution of nitrate of silver, say three grains to the ounce of water; or, with the nitrate of silver in substance; the former, however, being generally preferable, unless the ulcer is in a phagedenic or gangrenous condition, when the latter should take the place of the solution, as being more prompt and efficacious in its action.

The only eye-salve which I ever employ in this affection is the ointment of the nitrate of mercury, in a very dilute state; generally in the proportion of about ten grains to the drachm of prepared lard. The ointment of the shops is entirely too strong, and can never be used without the risk of materially augmenting the morbid action. Diluted in the manner above stated, it may be advantageously applied in all cases attended with great relaxation of the vessels of the affected part, opacity of the cornea, and adhesion of the lids. The proper way to use it is to annoint the edge of the lower lid with a small pencil, dipped in the salve, every night at bedtime. When the salve is stiff, it should be previously warmed, otherwise it will not be likely to adhere. Thus employed, a very small quantity, a portion not larger than half a grain of rice, will suffice.

Some patients experience great relief from bathing the forehead, face, and temples, frequently with warm water, pretty strongly impregnated with common salt; while others derive most benefit from bathing with cool, cold, or hot water. In all cases the best plan is to permit the patient to consult his own feelings in the use of this remedy.

It need hardly be added that the eyes should always be carefully protected with a green shade; but on no account should the patient be allowed to wear green glasses, or, what is still more abomina-

ble and injurious, goggles. Such a practice, indeed, cannot be too much deprecated. The same remark is applicable to compresses and bandages. I have seen numerous cases in which irreparable mischief has been done by the protracted use of these articles. The true, legitimate practice consists in protecting the affected organs in such a manner that, while they are sufficiently screened from the light to render the patient comfortable, they shall have the full benefit of cool air. As the morbid action declines, more and more light should gradually be admitted, until at length they receive their accustomed supply. It should never be forgotten that light is the natural stimulus of the eye, and that by withholding this stimulus for too long a time, the organ may become morbidly sensitive; just as the stomach becomes irritable and unable to perform its functions when it is for a long time deprived of food.

Finally, I may state to you that I have rarely derived any essential benefit, in the treatment of any form of scrofulous ophthalmia, from iodide of potassium, so much vaunted by some practitioners. Formerly I was in the habit of prescribing this article quite frequently, but it so often totally disappointed my expectations that I have, of late years, laid it entirely aside. In obstinate cases, you will occasionally obtain benefit, especially in weakly children, requiring an alterant and tonic, from the exhibition of bichloride of mercury, in very minute doses, as the twentieth or twenty-fifth of a grain, in union with Huxham's tincture of barks. I am well aware that the salt in this prescription undergoes some chemical change; but this renders it, perhaps, only the more efficacious. It is not necessary to carry the remedy to the extent of ptyalism to obtain its full effects. Indeed, such an occurrence should always be carefully avoided.

Where there is hemi-crania, or excessive circum-orbitary pain, anodynes are necessary, particularly at night, both to allay suffering and to procure sleep. Under such circumstances, some practitioners are in the habit of applying belladonna ointment to the affected parts, and in some cases I have found the remedy of service, but, in general, it has disappointed me.

During the latter stages of the disease, the patient should take, daily, gentle exercise in the open air, as a means powerfully calcu-

lated to improve his general health, and to invigorate his constitution. In all cases, the greatest care should be taken to avoid exposure and indulgence of the appetite and passions. As another powerful means of guarding against relapse, a moderate use of the remedies, above mentioned, should be persisted in for a considerable time after all disease has apparently vanished.

Case 2.—*Hydatid Tumor of the Arm; Excision; Cure.*

William Gar, a stout, healthy-looking Irishman, aged twenty-five years, applied at my office, a few days ago, on account of a small tumor on the right arm. When first noticed, about six months ago, it was not larger than a hazel-nut, rather hard, and free from pain. Its discovery seems to have been altogether accidental. The man is a common laborer; and, although rather intemperate, has always enjoyed good health. Supposing that the case would be of interest to you, I have requested him to come here to-day, and I now show you the affected part. It will be perceived that the tumor, which is of a rounded, elongated shape, occupies the outer part of the arm, and that it is apparently lodged in the substance of the deltoid muscle. It is about the volume of a large almond with its shell, somewhat movable, especially at its inferior extremity, and of a firm, fibrous consistence, with a slight degree of elasticity. In fact, the sensation which it imparts to the finger is very much like that of an indurated lymphatic ganglion; a circumstance which, were the site not unusual, would suggest the idea that it might be a body of that kind. Within the last two weeks, the tumor has been somewhat painful, especially after exercise, and the pressure made in examining it is productive of slight uneasiness. The skin is perfectly healthy, and there is no disease in the neighboring parts. It is proper to remark that the tumor is increasing in size, though not rapidly, and that the patient is not aware of any thing that could have produced it.

In regard to the diagnosis of this tumor, it is impossible to offer even a conjecture. If it were situated in a region of the body where lymphatic ganglions occur, I should unhesitatingly pronounce it to be nothing but a structure of this kind, in a state of induration and enlargement; but, inasmuch as this is not the

case, such a conclusion would be unwarrantable. That it is not a fatty tumor is apparent from its consistence and rapid progress. Growths of the latter description are always soft, especially when small, and of slow development. I can not suppose that it is a sebaceous tumor, because it is hard, and too irregular in its outline to justify such a belief, to say nothing of the fact that the sebaceous, like the adipous tumor, is always remarkably tardy in its formation. It may be an encrysted tumor; but here, again, we are met by the fact that there is an absence of some of the characteristics of this class of morbid growths, particularly elasticy and fluctuation. Nor is it likely to be malignant, and yet upon this subject it behooves me to be somewhat guarded. We may assume that it is benign from the circumstance, chiefly, that the man is not more than twenty-five years of age, and that there is an absence of the sharp, lancinating pain so conspicuous in malignant disease. Possibly, the growth may be fibrous; for it certainly has a good deal of the feel of such a tumor; but here, too, our diagnosis is at fault.

The above remarks, which, were the occasion suitable, might be greatly extended, will, I trust, not be lost upon you; inasmuch as they will serve to show you how imperfect are our means of diagnosis in certain forms and stages of morbid growths. If the tumor were not so firm, I might use the exploring needle; for, in this way, I might, perhaps, be able to ascertain the nature of its contents; but its solidity forbids such a mode of examination; and I shall, therefore, proceed to remove it; hoping, after a careful inspection, to be able to determine its structure, and to refer it to its appropriate head.

In performing the operation, I shall make a simple vertical inincision, when, reflecting the edges to their respective sides, I shall, probably, be able to enucleate the tumor by a rapid dissection, or merely by using the handle of the scalpel.

The above remarks having been made, the operation was proceeded with- In carrying the incision over the tumor, the knife penetrated it near its middle, and immediately there gushed out a small quantity of thin purulent fluid, inducing the belief that the swelling, after all, was merely a diseased lymphatic ganglion. Continuing the dissection, however, I soon found that there was a

distinct cyst, which, being collapsed, required some pains to separate it from the fibres of the deltoid muscle, in which it was embedded. After the removal was completed, Dr. Richardson, who had kindly assisted in the operation, picked up a small body from the floor, which, upon examination, proved to be a globular hydatid, not more than about six lines in diameter, and furnished with a distinct sucker. The cyst was very soft and thin, smooth internally, and filamentous externally. Hardly any blood was lost in the operation. The parts were approximated by adhesive strips; and, at the end of a week, the greater portion of the wound had united by the first intention.

Considering the great rarity of this form of tumors, it is not surprising that we should have been unable to determine its diagnosis before its removal. Only a few examples of a similar kind are upon record. In some of the internal organs, as the liver and ovary, hydatids are occasionally observed, both in the human subject and in the inferior animals; but they are very uncommon in the other viscera, and also in the cellular tissue, whether beneath the skin or among the muscles. Of their cause and mode of formation nothing is known. After having attained a certain age, they sometimes lose their ostality, and thus becoming a source of irritation, induce suppuration in the surrounding parts, followed occasionally by their elimination. The probability is that this process had already commenced in the case of Gar, as was evidenced by the escape of pus on puncturing the cyst in the dissection, although the hydatid itself was apparently unchanged. The great consolation, connected with the tumor, is, that it is benign in its character, and, therefore, unable to return.

Art. IV.—A CASE OF SECONDARY FŒTUS.

A LETTER TO PROF. MILLER, BY B. A. JESSE, M. D., OF SHELBY CO., KY.

Jesse's Store, *Shelby county Ky.*, Jan. 14th, 1854.

Professor Miller.

Dear Sir:—A circumstance so strange and unaccountable happened a few days since, in my obstetrical practice, that I wish to communicate the facts to you, for a solution of the mystery. I will go somewhat into detail, that you may have the entire history of the case.

A mulatto woman, a slave, about twenty-five years old, healthy, the mother of several children, was delivered on the 14th of April, 1853, of a healthy looking, male child, which died in a few days, of convulsions. She was delivered again the 11th day of the present month, of a still-born child of good size, with short and deformed legs and arms. Umbilical cord short, with an aneurismal sac about one or two inches from the umbilicus. The waters excessive in quantity, so much so, that she complained, for some time before her confinement, of pain and uneasiness from the distention. The labor was not protracted nor severe. The placenta delivered; and all seemed to be going on well. On the second day after her delivery I was sent for, to see her, "as there was something wrong." I went to see her; she said that the night before, "something came away," and it was attached to a cord or string, and she had tried to pull it away but could not. I examined her, and found suspended to the umbilical cord a fœtus of about six weeks or two months old. I traced the cord to the mouth of the uterus, but owing to the contracted state of the os uteri I did not attempt to insert my hand. I used very moderate traction on the cord and it parted from its attachment. There must have been another placenta, for I am satisfied that I extracted all that belonged to the first child.

Counting four weeks from the birth of her child in April, 1853, it would be about eight months to her last confinement; but I suppose any one judging merely from the size and appearance of the child would have believed she had gone full nine months. Can it be possible that conception took place in the sixth or seventh month of the first child? Was it a dwarf? Hardly, for it was not developed, not matured in its members, but just as we find a fœtus of six weeks or two months. I should be pleased to know whether or not you have met with any case similar; will you please favor me with your views in regard to it. I will add that the woman never had *twins* before, though I have delivered her of two children before this, similarly deformed.

Very Respectfully,

B. A. JESSE.

PROFESSOR MILLER'S REPLY.

Louisville, Jan. 20, 1854.

Dr. B. A. Jessee,

Dear Sir:—Your letter of the 14th inst., was duly received, and I thank you for the narrative it gives of a rare though not unique, case in obstetric practice. It has never happened to me to meet with such a case in my own clinical experience, but several such may be found on record. Not to refer to other authors, Dr. John Ramsbotham relates a few cases of the kind in his "*Practical Observations in Midwifery*," part ii., prefacing them by some explanatory remarks. He denominates, not very happily, as I think, the abortive product a *secondary fœtus*, and considers it to be twin of the living child, or the one that attains its maturity. In the cases related by him, there were symptoms of abortion at the period of pregnancy, when this is most liable to occur, but these symptoms subsided without the ovum's being expelled, and hence he inferred that the threatening of abortion was consequent to the loss of vitality on the part of one of the ova of a twin conception. The dead ovum may be retained in the cavity of the uterus till the living one acquires its complete development, and be expelled subsequently to it at the time of

parturition, as it happened in your case. You do not state whether your patient was threatened with abortion in the course of her pregnancy, and it would be interesting to make inquiries on this point.

That one of the ova in cases of twin pregnancy may be blighted without implicating the other, is proved by the observation that the dead ovum may be expelled, whilst the living one is retained, its development proceeding to maturity without molestation. The author, whom I have cited, gives cases of this kind also, in his valuable repository of facts, to which reference has been made.

If this explanation be not adopted, the only hypothesis which can be assumed to account for the phenomenon, is that of superfœtation, or a second and separate impregnation. I know of no sufficient evidence to authorize a belief in superfœtation, except perhaps its occasional occurrence in females with double uterus, or possibly in females with single uterus at a very early period, say a few hours, after the first impregnation. The changes that take place in the uterus shortly after conception and the establishment of a new process, that of gestation, are, as it appears to me, quite incompatible with a superimpregnation. I hold it to be altogether impossible at so advanced a period of pregnancy as the sixth or seventh month.

To my mind the most wonderful feature of the case reported by you, is the *superfœcundity* of the woman, whose uterine apartment was to let immediately, as it seems, after its evacuation on the 14th April, 1853, and was actually leased to him with the "short" limbs and his short-lived twin brother.

Respectfully, your obedient Serv'nt.

H. MILLER.

Art. V.—PHYSIOLOGICAL CHEMISTRY.

BY BENJAMIN SILLIMAN, JR., M.A., M.D., PROFESSOR OF MEDICAL CHEMISTRY AND TOXICOLOGY IN THE UNIVERSITY OF LOUISVILLE.

It is not our purpose to attempt a full review of the elaborate and truly learned works upon Physiological Chemistry whose titles are given below. * Such a review would, if sufficiently full to cover the whole ground, extend to an unreasonable length, and embrace details not interesting or even generally understood by the body of the medical profession.

Simon's Chemistry of Man, which appeared in 1842, marked an era in chemical physiology which has been followed and sustained by immense activity in researches bearing upon the chemistry of life. The results of the zeal and industry of the army of workers, which flocked to the laboratories of Paris, Giessen,

* Physiological Chemistry, by Professor C. G. Lehmann, translated from the second (German) edition, by Geo. E. Day, M. D., F. R. S., Fellow of the Royal College of Physicians, and Professor of Medicine in the University of St. Andrews. London: 1851, 1853. Printed for the Cavendish Society. Vols. I, II; 8vo., pp. 455 and 465, respectively

Atlas of Physiological Chemistry, (consisting of microscopic figures,) being a supplement to Lehmann's Physiological Chemistry, by Dr. Otto Funke, of the University of Leipsic. London: 1853; printed for the Cavendish Society; quarto, fifteen plates, (with ninety figures,) engraved on stone and printed in colors, with explanatory text by the author, quarto, pp. 29.

Robin et Verdeil Traite de Chimie Anatomique et Physiologique, Normale et Pathologique, &c. Treatise on Anatomical and Physiological Chemistry, Normal and Pathological; or, the proximate principles, both morbid and normal, which compose the bodies of man and mammiferous animals, by Chas. Robin and F. Verdeil; with an Atlas of forty-five plates, in part colored. 3 vols., 8vo., pp. 728, 584, and 595. Paris: J. B. Bailliere, 1853.

Lehrbuch der Zoochemie, von H. W. Heintz. Berlin: 1853; 1 vol., 8vo., pp. 1107, with two plates.

Utrecht, and Berlin, to follow up or refute the doctrines of LIEBIG, so eloquently announced in his well-known works,—these results, so long appearing as an intricate and unconnected mass of facts in organic chemistry, have now assumed a degree of systematic order and compact connection, worthy of the labor which they have cost.

We shall in the present article confine ourselves almost exclusively to the work of LEHMANN, trusting on a future occasion to resume the consideration of the others. LEHMANN's work has been translated and published for the members of the Cavendish Society.

The Cavendish Society is one of those organizations, established during the past few years in England, whose object is to publish, solely for subscribers, rare and important works, either historical or contemporaneous, which no publisher can be found willing to undertake. The Sydenham Society has thus conferred lasting benefits upon medical literature, by its now long catalogue of rare and valuable treatises. The Cavendish is doing the same service for the chemical reader. One of the most important benefits conferred by these associations is the translation, by competent hands, of the current medical literature of Germany;—researches published in German only, are a sealed record to the greater number of even well educated readers of English, and yet we know that the most laborious workers are Germans, and certainly some of the most original and important observations in all departments of science are made in Germany. The Cavendish Society employed Dr. GEO. E. DAY, F. R. S., to translate "LEHMANN's *Lehrbuch der Physiologischen Chemie.*" Dr. DAY's fitness for this task is evinced both by the ability with which he has rendered the original, and by his experience as the translator of SIMON's Chemistry.

Two only of the three volumes of LEHMANN's work have appeared in English, and the third were issued in Germany only toward the close of last year. It is accompanied by Dr. FUNKE's Atlas of Physiological Chemistry, a distinct work, but prepared with special reference to the elucidation of LEHMANN's Chemistry. The quarto plates of this work, fifteen in number, embrace ninety separate microscopic fields, exhibiting everything which can be seen in form, structure, or color, illustrative of the various mor-

phological elements and organic substrata of the animal organism, both in health and disease, as far as they have been observed by the author. Every detail is original and drawn from the microscope, and nothing is copied from other observers.

An apology for chemistry as the younger sister of medicine and claiming to be her teacher, is no longer demanded of the physiological chemist, and in the pendulation of professional opinion, the votary of this science is led almost to fear that the high expectations of the future development of a rational system of therapeutics, diagnosis and practice, as a result of zoochemical researches, may be met with disappointment. Not that he fears, or would limit the resources of his science, under the guidance of a rigorous induction from facts of undoubted authenticity; but, rather, as LEHMANN remarks, that "enthusiasm in the cause of organic chemistry has degenerated amongst many physiologists and physicians into fanaticism, which, even in the best cause, tends to invalidate a host of truths in its endeavors to uphold some single fact." We might be disposed to ask whether its most zealous partizans have not retarded, rather than accelerated, the period at which it will attain its proper share of appreciation, and its just recognition. In commencing, therefore, the study of physiologi cal chemistry, nothing is more important than clearly to understand the nature of the results which this department of science is now capable of yielding, and the requirements which, in its present state of development, it fulfils; and to ascertain the cause, the means, and the methods, most likely to lead us safely within its domain, and at the same time the best adapted to promote its further progress.

The subject of physiological chemistry naturally divides itself into three grand divisions—each capable of separate investigation, but following each other in a determinate logical sequence. These are, first, what are called the "organic substrata of the animal organism; i. e., such substances as are usually intended by the term "proximate principles," (and which is a translation of the French term *principes immédiats.*) Such are the various organic acids, the alkaloids, the salts of fatty acids (fats), sugar, coloring matters, protein compounds, (albumin, fibrin, etc.), and the various mineral constituents of the body. It is to the history of these,

as the corner stone of all rational knowledge of animal chemistry, that our author devotes his first volume.

The second division includes the "animal juices," regarded as the variable results of the union and secretion of the members of the first division, under the influence of the various processes of life-chemistry, occurring in the animal organism, both in health and disease. The discussion of this important class of products—which occupies the position of an intermediate link between the theory of the organic substrata and that of the zoo-chemical processes—fills the second volume.

The third division of the subject is the study of the zoo-chemical processes, based on zoo-chemistry and the theory of the animal juices—the theory of the metamorphosis of tissues, of nutrition and secretion, of digestion and respiration. This is the sum and crown of the whole subject, and fills the third volume of LEHMANN, which has not yet appeared in English, but is now in process of preparation by the Cavendish Society. Meanwhile, let the reader of English content himself with a careful study of the two volumes already in his hands, convinced that he cannot too thoroughly master the subjects therein treated, so fundamental to any rational and practical deductions which are to be drawn from them toward forming a just notion of the metamorphosis of the tissues.

LEHMANN's work is opened by an introduction, occupying twenty-four pages, which the author calls a "Methodological Introduction," and in which he discusses in a most masterly manner, and in the spirit of a cautious philosophy, the sure principles which should guide the scientific enquirer in the prosecution of physiological chemistry. From this "Introduction" we propose to make some rather ample extracts, as it is not possible to put the important truths it contains—as the ripe results of long study and great genius and skill—in more forcible and compact expression. We commend to the attention of men who are vainly striving after a "true theory" for the explanation of vital phenomena, not as the result of legitimate investigation, but as the off-spring of ingenious presumption, the following:—

"It is only by the application of abstract physical laws, by the establishment of certain momenta of empirically observed phe-

nomena, and by a steady adherence to safely guiding maxims,—in short, by logical sequence,—that we can advance in the investigation of vital phenomena. It would almost seem as if medicine, in the earlier periods of its history, had cast a shadow over those kindred sciences which are able to afford it aid and support, clouding even their brightest points. It has thus been found impracticable at once to rid medicine, notwithstanding its assumed physiological character, of the mania of attempting to explain everything by the old system of hypotheses; and hence this science has derived less benefit than many others from the exact method of physical enquiry, having simply borrowed certain materials from chemistry and the kindred natural sciences, and substituted, in the place of the older vagaries of natural philosophy, various chemical phrases and high sounding terms, scarcely less devoid of true import than the former. This deficiency in logical sequence, which we so frequently at present encounter in medicine, has unfortunately also infected animal chemistry; for here likewise facts have not been sufficiently distinguished from hypotheses, or hypothesis from fiction. This is more easily accounted for in physiological than in pure general chemistry; for while the latter treats almost exclusively of palpable phenomena and of well-established facts, which easily admit of being reduced to definite laws, in the former we must necessarily have recourse to experiments and natural investigations, whose success must in a great measure depend on individual operations of the mind. Zoo-chemical processes are the most complicated of any comprised in the domain of natural enquiry; but such processes are not capable of tangible demonstration, but must be divined, or rather, intellectually apprehended. Our senses are incapable of perceiving the causal connexion of things, or the logical succession of phenomena; thus we do not see motion, but simply recognize it by the result of the changes effected by it; we do not perceive heat, but simply the variations of the temperature, and the results to which they give rise, &c. Hence it is not our senses which here deceive us, but the judgment which we form regarding the objects presented to us by the perceptive faculties. The causal connexion of several allied phenomena, (*i. e.*, a process,) can therefore only be comprehended by the subjective combination of individual objects perceived by the senses, and not by sensuous intuition alone. But as soon as we subject to investigation the highly complicated chemical phenomena of life, we enter upon the actual domain of hypothesis. It unfortunately happens, however, that the correct logical conception of an hypothesis has been completely lost sight of, and its place supplied by the vaguest fictions; whence the term has fallen into such discredit that many have been desirous of setting aside all hypotheses, unmindful that even the simplest form of experiment cannot be prosecuted without their aid. Hypotheses

are indispensable in every physical enquiry, and must constitute the base of every experiment, as they are in fact merely the subjection of our thoughts and mode of intuition to the reality of phenomena. The question, however, always is, whether the facts at our command logically justify such a procedure, since where such is not the case, the deduction at which we arrive is undeserving the name of an hypothesis, and is a mere fiction, supported at best on a hypothetical foundation.

Physiological chemistry has given rise to many delusions of this nature, owing to its imperfect development, and to the necessity presented by physiology and pathology for chemical elucidation. Some few isolated deductions were drawn from superficial chemical experiments, and arranged in a purely imaginary connexion by the aid of chemical symbols and formulæ, for whose establishment analysis in many cases did not even afford any sanction. Thus, for instance, in the attempt to form a conclusion regarding the metamorphosis of the blood from an elementary analysis of its solid residue and of the composition of the individual constituents of the excretions, there is an utter absence of all scientific groundwork; for, independently of the fact that the elementary analysis of so compound a matter as the blood is incapable of yielding any reliable results, and cannot, therefore, justify the adoption of any special chemical formulæ, it is assuredly most illogical to attempt to compare the composition of the blood collectively, with that of the separate excrementitious matters. In such deductions, expressed by chemical formulæ, the addition of atoms of oxygen, and the subtraction of those of water, carbonic acid, and ammonia are wholly arbitrary: for chemical analyses do not afford the slightest grounds for the majority of these equations. When, on the other hand, we have seen uric acid decomposed by different oxidising agents into urea and other bodies, and when, further, we find the quantity of uric acid increased in the urine in those cases where a diminished quantity of oxygen is proved to be contained in the blood, we are justified in concluding that also in the animal organism a portion, at least, of the urea found in the urine must have been produced by the oxidation of the uric acid. In the formulæ which expresses this deduction, we have an hypothesis, but a well-grounded one, which, although requiring further confirmation, is yet wholly different from the frequently condemned, but rarely avoided, abuse of chemical symbols. Chemical equations having no other foundation than the presumed infallibility of empirical formulæ, must, however, cause us to deviate from the path of physical enquiry, and involve us in a chaos of the most untenable delusions. Thus, for instance, a chemical equation might lead us to conclude that glycine (glycocoll) was the source of urea and lactic acid in the metamorphosis of the animal tissues; for we might conclude that two equivalents

of hydrate of glycine were decomposed into the above-named substances according to the formula, $C_8 H_{10} N_2 O_8 = C_2 H_4 N_2 O_2 + C_6 H_5 O_5 . H O$. All experiments hitherto instituted with glycine are, nevertheless, opposed to such a disintegration. If, then, we would deduce urea and lactic acid from glycine, which has not been proved to exist in the blood, we should be neglecting the most comprehensive rule of logic, according to which one hypothesis cannot be supported by another. It has, however, unfortunately been too much the practice in recent times to employ far more complicated equations as supports for such purely subjective modes of contemplation, by which a semblance of the most exact method of investigation has been assumed. By these means a number of chemical fictions have supplanted the fancies of that speculative natural philosophy which in earlier times encumbered the study of physiology and pathology, and have plunged medicine into the midst of a new labyrinth of untenable theories."

The author then proceeds to indicate a further cause of the partial failure of the application of chemistry in vital phenomena, in the imperfect causal connexion among the different branches of natural science. He illustrates this by reference to the diagnosis afforded by pathological anatomy, which has received so little attention on the part of those who have conducted pathologico-chemical enquiries, while on the other hand the adherents of the pathologico-anatomical school have made full use of chemical phrases and fictions without an adequate acquaintance with the general science of chemistry. If chemical investigations, regarding objects belonging to pathological anatomy, would aspire to a scientific value, and if they are to afford any true elucidation of pathological processes, it will assuredly be admitted that the question should be adequately considered from an anatomical and diagnostic point of view. Yet every day presents us with instances of the most flagrant neglect of this self-evident proposition. How frequently we hear of the chemical examination of diseased bones, without any regard to a diagnosis at all in accordance with the present condition of pathological anatomy. We even more frequently meet with similar inconsistencies in the investigation of diseased animal fluids.

We cannot read the following bold and manly sentences without wishing that a similar courage in facing the truth could animate all teachers and investigators—so strongly contrasted with the too

prevalent desire evident in many writers to slur over the weak points of their subjects with the semblance of fact or argument in hope of making a strong case.

"A third circumstance which has led to misconceptions in physiological chemistry depends upon an over-estimate of the value of chemical auxiliaries, and a complets ignorance of the present condition of organic chemistry. Have the numerous analyses of morbid blood, instituted during the last few years, fulfilled the expectations of physicians? With all due gratitude to the indefatigable investigators who, with no other aid than that which zoochemistry could offer, boldly attempted to throw light on those obscure enquiries, it must be admitted that, when we seriously enquire into the recompense of all their labours and sacrifices, we find that the result, although too dearly bought, was altogether inadequate to satisfy the requirements of pathology. Have the numerous analyses of urine led to much more than the assumption of several new species of disease, or so-called diatheses? Although we might have anticipated greater results, we can hardly wonder that the efforts hitherto made should either wholly or partially have deceived our expectations; for although these investigations may have rendered chemistry no unworthy auxiliary to a physical diagnosis, analyses of morbid products could hardly afford an insight into the chemical laboratory of the organism, while the means were wanting to prosecute them with the scientific accuracy attainable in the case of mineral analyses. Animal chemistry is still wholly unable to afford us a precise, and at the same time a practically useful method of investigating the blood; and how should it be otherwise while we continue to be in doubt regarding the chemical nature of its ordinary constituents? The mineral substances of normal blood are not yet determined, or, at all events continue to be made the subject of dispute; we scarcely know the names of the fatty matters it contains; one of its most important constituents, fibrin, cannot be chemically exhibited in a pure state; we are ignorant of the nature and mode of secretion of the globulin of the blood-corpuscles; we are still far from being able to separate and determine the so called protein oxides; and we are also ignorant of the excrementitious substances occurring in the blood. How then, amidst these and a thousand other uncertainties and doubts, can an investigation of the blood be scientifically and trustworthily conducted? We analyse healthy and morbid milk, and yet we are ignorant of the substances whose admixture we have termed casein. The urine, in its morbid condition, presents many varieties; and yet our knowledge of this secretion, frequently as it has been analysed, amounts to little more than an acquaintance with the quantitative relations of some of its principal constituents; creatinine and hippuric acid have not been de-

termined by any analysis, and doubts are still entertained by some chemists, (although most unjustly,) regarding the presence of the latter in human urine, while absolutely nothing is known regarding the most important pigment of this secretion. Many experiments have been made and theories broached on nutrition and digestion, and yet to almost the present day the existence of lactic acid in the gastric juice has been contested. Although hypotheses are not wanting regarding the mode of action of pepsin, we know nothing of its chemical nature, and we are wholly ignorant of the proximate metamorphosis of albuminous bodies in the stomach during the process of digestion. Will Mulder be able, even with his most accurate analyses, to support his protein theory by the aid of sulphamide and phosphamide? or is this term destined merely to indicate a past epoch of organic chemistry? When such is the state of animal chemistry, can we wonder that there should be obscurity regarding the chemical processes in the animal body, their various isolated and combined actions, their causal connexion and their dependence on external influences and internal conditions? Unfortunately, we might be led to believe, from the lectures and writings of many physicians, that, trusting to the aphoristic and often highly apodictic assertions of certain chemists they felt secure of having reached the object of their enquiries. Although at present little more than the direction is indicated we may hope in due time, and after innumerable efforts, to see our endeavors crowned with success."

Having enumerated the deficiencies and errors belonging to the chemistry of the vital processes, the author passes to the method and principles by which alone this science can be made to fulfil its just requirements. The final result of all physiologico-chemical investigations is assuredly to gain an accurate knowledge of the progress and connexion of the chemical phenomena attending the vital processes. It is needless to prove that we must thoroughly understand the substrata of the metamorphosis of the animal tissues before we can venture an opinion on the nature of the processes. The surest supports of physiological chemistry are to be sought, therefore, in general organic chemistry; while the study of the organic substrata of the animal body, must necessarily constitute an integral part of physiological chemistry, and prove a most efficient aid towards its development.

After having in this manner familiarized ourselves with the organic substrata of the animal body, we are still only on the threshold of the study of the constitution and functions of the animal juices and tissues.

"The study of the zoo-chemical processes based on zoo-chemistry and the theory of the animal juices, appertains to the third section of physiological chemistry, the theory of the metamorphosis of tissues—of nutrition and secretion. It has already been observed, that the actual object of physiological chemistry is to examine the course of the chemical phenomena of the animal organism in their causal connexion, and to deduce them from known physical and chemical laws; or in other words, to explain them scientifically. Even if we regard the chemical substratum, as made known to us by zoo-chemistry and the theory of the juices in the light of a satisfactorily investigated question, there are still several directions to be pursued before we can reach the proper object of our enquiries. It is here most essential that we should be well acquainted with the paths to be followed, for in our search after truth we are compelled to call to our aid hypotheses which might easily lead us into the domain of pure fiction.

"As long as zoo-chemistry and the theory of the juices continue to occupy their present subordinate position, the only method by which the foundation necessary to an exact investigation can be obtained, is that which we may term the statistical. LIEBIG, BOUSSINGAULT, and VALETIN have indeed, with a more correct view of what was required, attempted to compare the final effects of the whole with the material substrata supplied to the organism. We cannot, it is true, arrive at any conclusion regarding the working of the process itself by a mere juxtaposition and quantitative comparison of the ingesta and excreta of the animal organism, any more than we can judge of the causes and course of diseases by the number of fatal cases recorded; but such experiments furnish us with certain general results which serve as guides to further investigations. Some of the most important questions, whose solution was specially necessary, were unanswerable by any other method. Thus, for instance, it was ascertained, by an accurate investigation of the food, and its comparison with the constituents of the excreta and of the nutrient fluids, that in the ordinary food of animals, albuminous substances occur in sufficient quantity to compensate for the nitrogenous matters lost in the process of nutrition and in the metamorphosis of tissue; while it was thus at the same time shown, that the animal organism does not necessarily possess the property of generating albuminous matter from other substances containing nitrogen. The question whether the animal organism possessed the property of generating fat was answered in a similar manner; and it is well known that by means of such statistical observations, (comparing the fat contained in the food with that secreted in the cellular tissue and mixed with the excrements) the contest carried on between LIEBIG on the one side, and DUMAS and BOUSSINGAULT on the other, regarding the formation of fat, was finally decided in favor of the former.

"This statistical method preserves us from setting up untenable hypotheses, and prosecuting useless experiments. How long were the minds of natural philosophers haunted with the illusion that animal bodies possessed the power of generating mineral elements, as lime, iron, sulphur, &c., from other elements, or even from nothing! It was this method alone which exposed the perfect nullity of the obstinately defended dogma of the 'vital force.'"

The author follows up this line of argument by allusion to other applications of the statistical method as one of the most important aids toward a solution of some of the more general questions in reference to the metamorphosis of the animal tissues. In addition he refers to the *comparative analytical* or *chemico-experimental* method, by which he may examine the cause of phenomena and the cause of their succession, in as far as the chemical phenomena of the living body may be artificially imitated, and the chemical metamorphoses of certain substances, external to the vital sphere, be compared with those within the influence of the vital processes. Liebig and his school have here done essential service.

"Liebig was led to believe from his statistical enquiries on fats, that these substances in their transmission through the organism, were in a great measure oxidised and reduced to water and carbonic acid, by which means they specially contributed towards the maintenance of animal heat. As he was by no means inclined to believe, as some have supposed, that fat was consumed in the lungs, somewhat in the same manner as oil burns in a lamp, it was necessary more accurately to investigate its gradual metamorphosis, and its transition through different stages of oxidation, and into bodies containing a larger quantity of oxygen. He believed that he could most readily attain this object by the comparative analytical method; and hence he and his school entered upon a series of experiments on the numerous products of decomposition of fatty matters, and more especially on their products of oxidation; and although we may still be far removed from the object in view, these enquiries have enriched us with many valuable results. A similar instance is afforded by the gelatigenous tissues of the animal body; for although our histological and statistico-chemical investigations leave not the slightest doubt that the gelatin is formed from the albuminous matters, the process of this metamorphosis is still wholly unexplained; and before we shall be justified in forming an opinion regarding this metamorphosis, and expressing it by a chemical equation, it is indispensably necessary that we should investigate the metamorphoses experienced by albuminous bodies during their gradual oxidation.

We are indebted for these views to the admirable investigations prosecuted under LIEBIG's direction, by SCHLIEPER and GUCKELBERGER, on the products of oxidation of albuminous bodies and of gelatin."

We have no wish to trespass on the patience of our readers by extending our quotations from LEHMANN to an unreadable length, and we will therefore content ourselves by giving a few sentences from the concluding paragraph of his "Introduction."

"In conclusion, we would advance a few remarks on the place which physiological chemistry occupies, or at some future period will occupy, among the auxiliary medical sciences. If the final result of all physiologico-chemical enquiries be that of comprehending the chemical phenomena of animal life in their different phases and in their causal connexions, it is obvious that we must look to this science for a solution of the most important questions of physiology, and of medicine generally. It cannot be denied that most of the phenomena of animal life either consist in or are accompanied by chemical processes; nor can we form an adequate conception of the functions of the nervous system by which sensuous perception and motion are regulated, without the simultaneous existence of chemical actions. For although we are as yet unable to make nervous action fully harmonise with definite physiological laws, or to identify it with certain physical forces or imponderable fluids, all physiological experiments indicate that it is always followed by a chemical reaction, and that the nervous system experiences chemical changes by and through its own activity. It must, indeed, be admitted that any actual proof of such chemical metamorphoses is at present perfectly unattainable, and that our chemical methods would here afford us no higher aid than that which the scalpel yields to the pathological anatomist. But ought we to despair of attaiding our object, because we do not as yet clearly perceive the direction we are to follow? Weariness of the senses is the diminished impressibility of the nerves of sense, but its cause cannot reasonably be sought for in any other than a chemical change, experienced by the conducting substance of the nerves. Such a chemical metamorphosis of the nerves of sense from external impressions can no longer greatly excite our astonishment, since we have witnessed the unexpected phenomenon of a picture produced suddenly, and as it were by magic, from the chemical changes effected by the rays of light on an iodised silver plate. Should we not be equally justified in saying that the iodised plate, which after being exposed for a few seconds to a strong light gives only faint and half effaced images, is wearied like the retina, when after repeated and continuous perception of an image, it gives back only the faint outlines of the object? We

may rest assured that the nervous system is not exempt from chemical action; and if the nervous system itself must fall within the domain of chemical contemplation, and a chemical expression remains to be found for its action, no less than for that of digestion and for the formation of blood, it is scarcely necessary to offer further proof of the fact that chemistry is destined to play the most important part in physiology and medicine. However much we may endeavor to exclude chemistry from certain physiological investigations, we shall always find that it involuntarily forces itself upon our notice; for without it we shall be unable to find a physiological equation or a philosophical expression for a process. In a scientific point of view chemistry must, therefore, be regarded as an invaluable acquisition to physiology. We have, then, little cause to dread that Cicero's observation "*Suo quisque studio delectatus alterum contemnit*," will be applied to ourselves when we assert that physiological chemistry is the crowning point of every physiological enquiry.

"When we turn to practical physiology, to pathology, and therapeutics, we are again reminded that chemistry is indispensable. Is there a single disease that is not attended by chemical changes? Can we ever hope to comprehend or explain the nature of any process, if we are ignorant of its integral factors? Life cannot exist without chemical movements, disease cannot exist without chemical changes. Thus much in reference to pathology; while in respect to therapeutics, it is almost superflous to observe that chemistry here also plays the principal part, for where has modern pharmacology sought its chief support, save in chemical processes and principles? And if we have advanced so far towards a clear insight as no longer to ascribe supernatural forces to medicines, but to derive their efficiency especially from chemical properties, then must chemistry be the supporting basis of pharmacology. The physician acts upon the body mostly by the aid of matter, which retains its characteristic powers within no less than without the organism. If then nervous action likewise falls within the sphere of chemical metamorphoses, the *Nervina* (or *Neurotica*) of pharmacologists must primarily at least act chemically on this system."

We are reminded by this argument of our author, of a remarkable passage in one of the lectures of Dr. John Simon, F. R. S., of London, on general pathology. He is speaking of the use of excretive drugs in relation to humoral pathology, (lecture xi.,) and after announcing that "within the compass of pathology, I know of nothing so obscure and unsatisfactory as that division of the science which relates to morbid and medicinal changes in the excretory functions:" he proceeds to contrast the vagueness and

uncertainty of our knowledge of the action of remedial agents with what occurs in chemistry and physics—and discourses in the following remarkable and suggestive manner:

"We are led to an intimate pharmaceutical acquaintance with the *Materia Medica*. We learn the smell of rhubarb, the tests of arsenic, the chemical incompatibilities of iron, the adulterations of quinine or sarsaparilla, and so forth. We become skilful dispensers. We know that bark very often cures ague; that copaiba frequently stays a clap; that cod-liver oil and iodide of potassium between them relieve a good many cases of scrofula; that taraxacum is sometimes useful below the diaphragm, and squill above it. And with this farrago of traditions, the misnamed *science of Materia Medica* has remained so contented and so stationary, that at the present moment—in the middle of the nineteenth century—we do not possess a complete medical knowledge of any single article of the Pharmacopœia.

"It would be easy for one quite ignorant of medicine to predict, on general grounds, what must be the result of this state of things; what blind reliance on specifics where they are supposed to prevail; what credulous—perhaps rather what desperate combinations of drugs where we have no traditional specific to trust to. An indifferent shot in a turnip-field does not mark his one bird and bring it down; he bangs at the whole covey, and thinks it a hard case if he hits nothing. On some such principle as this—scarcely sanctioned in the sportsman, much less admissible in the medical philosopher—we too often discharge our barrels. If the immemorable *certificates* of a specific serve us instead of real knowledge, and enable us confidently to give quinine to one patient, and sulphur-ointment to another—good; but in the absence of such traditions, how many prescriptions are written, which combine at least a dozen different materials, where the prescriber is utterly unable to define the precise errand on which any one of these ingredients is sent into his patient's body. This prescription failing another of the same sort is written, and another after that; and presently the patient dies or recovers, as the case may be. And in either event, from the absence of a definite principle in the treatment, and from the want of having attached a separate meaning and intention to each drug employed, the pharmaceutical experiment has been too complex for the practitioner to draw any rational or useful inference as to the immediate causes of his success or failure.

"Contrast this with what occurs in chemistry or in physics. You wish to precipitate a certain material from its solution; you do not throw in at random the contents of several vials, with a vague hope that one of these re-agents may answer your purpose; you proceed with a very definite object, and by appropriate means.

If A be present (you say) B will throw it down, by forming such or such insoluble combination—provided (you may perhaps have to add) that C be not present likewise, for in that case C must, first of all, be neutralized; provided also, (you may further have to concede) that B be added in a sufficient, and not in an excessive quantity, lest it re-dissolve its own precipitate. These, or similar allowances being made, the sources of fallacy are just as definite and palpable as the anticipated positive result; and, if your operation should miscarry, you would be able to account for its failure, and to repeat it in a form which—not casually, but certainly—would accomplish your purpose. So, again, if you plunge the wires of your galvanic battery into water, and fail to produce the expected result, you do not fly off to some new arrangement—you do not try wooden conductors, or add twelve additional wires, or attempt to decompose the fluid with your finger and thumb; you know exactly what the hitch must be, that your circuit must be incomplete, or that you have forgotten the acid, or something of this kind; you correct your omission, and you are certain of the result—*certain*, because you know definitely the means to attain it, and are cognizant of their method and principle of operation. On the other hand, when we have given colchicum to six patients suffering with gout, when it has relieved two of them, and has left four unrelieved, we are quite unable to explain why it failed here, and succeeded there; and consequently, in that unsuccessful majority, we are quite unable to supply the absent condition (whatever it may have been) which was accidentally present in the successful instances, and conduced essentially to their cure.

"It is quite indispensable for the progress of medicine—I might say, indispensable for its existence as a science—that our *Materia Medica* should be made subject to a true pharmacology; that its province should cease to be a mere emporium of recipes; that we should have a knowledge of its various elements in their true relation to the living body; that we should give drugs only with a clear perception of their causativeness, and with a definite object before us; understanding the medicine as well as the malady, and taking one good aim at the substance of the disease, instead of discharging a volley at its shadow."

After showing the close and inseperable union which must subsist between pathological and physiological chemistry—and the absurdity of rejecting the evidence of the former on the ground that it deals with abnormal conditions—our author lastly enters a warm protest against the transcendental doctrines of what is called metaphysiology. The recent advances of organic chemistry have unfortunately been interwoven with a fantastic physiology, which designates itself as a comparative science. Comparing together

the *functions* of the organs of different animals as comparative anatomy compares their structure, is not a science, but a system founded on abstractions and ideal comparisons. We entertain all due respect for that form of metaphysics which occupies the same rank among the speculative sciences which chemistry and physiology hold among the exact sciences. But metaphysics and physiology may be likened to two divergent lines, which coincide only in their starting point, never again to unite. "We therefore believe with the diffidence beseeming true students of nature, that it would be wiser, and more conducive to the spread of true knowledge, to adhere in the study of vital processes, to matter and to the laws by which it is determined, than, following the fictitious abstractions of dynamical processes to assume that there exists in life a higher power of the spiritual force pervading matter.

"We are firmly convinced that even metaphysiology will be unable to deprive physiological chemistry of the consideration due to it among physical studies, in its explanation of vital processes; and we will, therefore, leave it to the poetic and the imaginative to depict the romance of the protecting activity and sturdy contest maintained by the vital force, and of a struggle between different powers,—between the attraction and repulsion of polarities. Does it not need a superabundant richness of fancy to believe with metaphysiologists, that apparent death, trance, or (as it has been termed) latent life, is the predominance of the spiritual over the material (the metamorphosis of matter being at its minimum) rather than a predominance of the material over the spiritual, as sounder minds would be led to assume? It would be well if these spiritualists would look down from the high stand they have chosen, and deign to believe that there are some among those experimentalists, who, clinging to matter, and gathering their facts with ant-like industry from the lowly earth, notwithstanding that they have long held communion with the poet-philosopher, Plato, and the philosophical natural inquirer, Aristotle, and have some familiarity with the Paraphrases of Hegel and Schelling, are yet unwilling to relinquish their less elevated position. If these happy admirers of their own Ideal had descended from their airy heights and closely examined organic matter, they would not have deemed it necessary to assume, that besides carbon, hydrogen, nitrogen, and oxygen, organic substances must also contain an *organogenium* or latent vital force, or whatever else they may be pleased to call it. Had they sought information from a chemist, they would have learnt, that when exposed to the clear light of rigid logic, there is no essential difference between organic and inorganic

bodies; a chemist totally unacquainted with organic matter, would *a priori* have deduced all these incidental differences of matter, from the doctrine of affinity and the science of stoichiometry, evolved from dead matter. However these advocates of a romantic poetry of nature may despise the swarm of industrious investigators, who are often unwearyingly occupied for years together in endeavoring to collect a few firm supports for the great edifice of a true philosophy of nature, we do not despair of seeing our work rise in simple grandeur, more durable and lasting than those sophisms of natural philosophy which, passing through ages from Pythagoras and Empedocles to Schelling and Hegel, have, like the sand of the ocean shore, been alternately upborne by one wave and engulphed by the next."

Having thus followed our author to the threshold of his subject we must bring our article to a close and wait the publication of his final volume on the transformation of the tissues and the great vital processes, satisfied if we have been able to indicate clearly the line of his argument and the spirit of safe, cautious and practical philosophy which animates his work. We trust that an American reprint of Lehmann's Chemistry will be undertaken in order that the work, now comparatively inaccessible and very costly, may be brought within the reach of all students.

Robin and Verdeil (*Chimie Anatomique*) have produced a work of great labor, covering much the same ground as Lehmann. It is elaborated in the best style of the French school, and in respect to its thoroughness of bibliographical and literary research leaves nothing to be desired. It is entirely out of the question that we should enter at all on its merits at present. The authors are well known as among the most laborious, earnest workers in "young France." This present treatise is accompanied by an atlas of microscopic plates of the crystalline bodies found in the fluids and cavities of the animal organism, drawn and colored from nature, by the authors, which surpasses everything ever done before in the same line, not even excepting the faithful excellence of Dr. Funke.

The discovery by Verdeil of a new and peculiar acid (the *pneumic*) in the substance of the lungs has led to the broaching of a novel theory or explanation, for the origin of the carbonic

acid of respiration, as being the product of the action of this acid upon the alkaline bicarbonatis of the blood. This new view, so much at variance with the "combustion theory," is discussed at some length, and with much ingenuity. Taking the facts as they are stated, it shows how far we yet may be from having conquered the first foot of stable ground, in the still new domain of this most vital function.

Editorial Department.

CHOLERA.

This plague appears to be again impending over us. Cases of it have occurred on nearly all the vessels that have arrived for some months past, at New Orleans and New York, from the ports of Europe. The disease has never been epidemic in our country during the winter season, but there is reason to apprehend that on the return of warm weather it will again spread from those points.

It is a melancholy truth that the profession is nearly as much at a loss with regard to the treatment of Cholera as it was at the end of the first epidemic. All are agreed that the complaint is manageable in its first stage, but when it has become confirmed Cholera, we have to confess the inefficacy of all the modes of treatment hitherto adopted in the disease. But certain facts of great hygienic value have been established by the successive returns of the epidemic. We think one of these is, that the poison which generates it is portable. We do not think this admits of a question. Another is, that the places visited by it once are those which are most liable to suffer on its return. Not only the towns

and villages which have experienced it, but the particular localities in such places, and even the very houses, it is said, are those in which it is now reappearing. A third is, that evacuating such houses and neighborhoods often puts a stop to the disease. Crowding seems to be one of the most efficient of its exciting causes. The obvious policy therefore is, if the plague should appear, to remove from the infected house or district. In all places where the epidemic has prevailed, there have been *foci* where it has manifested peculiar intensity. The inhabitants of such neighborhoods ought to fly at the approach of the pestilence. We have seen the happiest effects, in the country, from dispersing the negroes on a plantation when the disease broke out among them. On sending them into the woods, into temporary sheds, we have repeatedly witnessed the subsidence of the complaint.

It is well ascertained that the night air of infected districts is especially pestilent. Physicians attend the sick during the day with impunity, but nurses and those who sleep in them are exceedingly apt to be attacked. We have had cholera in Louisville repeatedly, but it has always exhibited its chief severity in a few limited districts. It has never been general in the city. Now, it appears to us, nothing can be more rational than to quit those districts, if the plague should again visit them. Filth, undoubtedly, is favorable to the spread of cholera, and we cannot be too much on the alert to remove all such sources of the poison. But we should rely much more upon flight. Removal to a very short distance is often sufficient. When the pestilence visited us last, in the summer of 1851, its ravages were almost wholly confined to the corners of four squares at the end of one of our market houses. All other parts of the city were almost entirely exempt,

EPIDEMIC OF 1853 IN NEW ORLEANS.

THE REPORT of the "Howard Association of New Orleans," contains some instructive statistics in relation to the epidemic yellow fever which lately desolated that city. Thus, it appears that of 11,088 cases which came under the care of the Association, 5,845, or more than one-half, occurred in natives of Ireland, while

only 716 were afforded by our native population. Among Germans there were 2,890 cases; 198 in natives of England; 56 Scotchmen; 436 Frenchmen, and small numbers among the natives of most other countries. Of the whole number 5,203 were males, and 5,885 were females; 9,415 were adults, and 1,673 under sixteen years of age. The number that died was 2,942, and of those discharged cured 8,146.

The amount of money disbursed by the Association was $159,190 32 and the amount contributed $228,927 46, leaving a large balance in their hands for future charities. We remark that of this large sum expended, less than $15,000 went to Physicians for professional services, while the medicines cost over $19,000, and the amount paid for nursing patients at their residences was a little more than $25,120.

The Association give a most satisfactory account of their stewardship, and their Report will everywhere be read with interest. They say justly, that the example of fraternal fidelity and devotedness exhibited by our people on this memorable occasion is one which has few parallels in the history of men.

THE LATE PROFESSOR HORNER.

Professor Jackson, in his introductory lecture delivered in the University of Pennsylvania, at the opening of the present session, commemorative of the late Dr. Wm. E. Horner, details some interesting and affecting incidents in the life of that eminent teacher and physician. He says:—

"It would not be doing justice to Dr. Horner, or give a correct idea of his energy, self-command, and indomitable resolution, or a true conception of the disadvantages under which he labored, the long unceasing struggles he sustained in his progress, and the heavy cost at which his success was attained, did I not reveal a peculiarity of temperament, or psychical idiosyncracy, never observed or suspected to exist, by his most intimate associates and friends, or even by a large portion of his family circle.

"While his exterior life appeared clear, bright, calm and prosperous, his interior life was dark, desponding, agitated with vague apprehensions, and every mental effort a conflict, a struggle, and a victory."

Prof. Horner, in a journal which he kept for a number of years, quoted by his biographer, thus describes his "constitution-"

"It was considered at school that I learned with facility; but I never believed it. I have had headache or dull pains in the head three-fourths of my waking life, seldom acute, but always such as to make me uncomfortable, and prefer solitude to company.

"Short intermissions of this state of suffering have occurred. I have then felt illuminated as the earth is when the sun emerges from behind a cloud. I have then hoped for a pleasurable existence, but it proved delusive, and I quickly relapsed into my ordinary state. Considering this serious obstacle to mental improvement, I wonder how I have made any advances, and especially such as to have given me an honorable station among men."

In another place he says:—

"My spirits get into so deplorable and hypochondriac a state that I have a thousand times thought death would be a most welcome visiter, and have almost envied those whom I have just heard to have passed from the bondage and anxieties of this life.

"As I grow older, my system is evidently getting more and more under the influence of the preceding causes. From the smallest article of food used in the evening, the next morning I am rendered uncomfortable in the extreme; my mental faculties are hebetated, and I am so vertiginous as scarcely to be able to collect my ideas or go on with a demonstration. The latter state has indeed become so constant and frequent, that I have frequently thought my labors as a public teacher were becoming too imperfect and confused to deserve respect, and that it would be better, perhaps, for me to retire and seek for some other occupation."

He wrote thus in his journal, in 1829:—

"I go to bed dissatisfied, taciturn, and looking for no greater comfort on the day to come, than I have enjoyed during the day past. Such is the unprepossessing picture of my life at the present time, and such has it been during the last six weeks; enjoyment has ceased, happiness has fled; I am inactive, worthless, lethargic."

And a few weeks afterwards he wrote as follows:

"I find it vain to resist the current of one's nature. I am at the present moment, just as I have been for the last four months, a confirmed and dissatisfied *ennuyé*. Discontented with myself and feeling no pleasure or satisfaction in the things around me, and finding every plan abortive, either in study, religion or amusement, from which I hoped to obtain that steady and enduring

quietude of mind, which I have on former occasions enjoyed. I must now make up my mind to move down the current of life on those terms that destiny, my peculiar nature, and my particular pursuits seem to have imposed unchangeably on me. I thank my Creator for the many unmerited favors I have received, and am constantly receiving at his hands. I ask pardon and forgiveness for the ingratitude of my nature, which prevents my mind from being illuminated with a single ray of joy, in reflecting on all His goodness. In the midst of the means of happiness, I am the victim of an unhappy destiny; my mind is cast in a mould which makes it insensible to the best gifts of Providence; and all that remains for me, is to submit resignedly during the remainder of the voyage, now drawing to a close, down the overflowing stream of time."

And yet, as remarked by Prof. Jackson, "the unhappy and disabling affliction," recorded in these memoranda, "was manifested by no exterior sign," and the fact that Dr. Horner was the subject of such a melancholy will take by surprise those who were accustomed to see him in his daily walk. Of course, the source of his mental disquietude was some physical derangement—some morbid condition, most probably, of the brain, which was only a little less intense than that which drove the amiable Cowper to the brink of insanity. It shows the heroic character of Dr. Horner's mind, that he was able to go forward under such a gloom in the discharge of his manifold duties without a murmur or one word of complaining.

HOMŒOPATHY.

It is a question with many persons whether it is good policy in our profession to notice the medical delusions that are continually springing up; whether a consequence greater than they would otherwise attain is not given to them by the opposition of scientific physicians. There are those who would leave them, as the traveller does the noisy insects in his path, to die a natural death. They succeed each other rapidly, to flourish for a season, and then pass into the limbo of forgotten things. But another party would pursue a different course. They insist upon the expediency of exposing each new humbug as it is blown into existence; and for

ourselves, we confess, we incline, not exactly to the belligerent system, but to giving the people light. If we as a profession withhold it, from what quarter can it come? Unless the false systems ever and anon arising are exposed by medical men, how are they to be exploded?

This view of the matter is taken by PROF. SIMPSON, who, deeming a further and fuller exposure of Homœopathy expedient, has written the most elaborate refutation that the popular delusion has yet called forth. The following capital anecdote is given, and illustrates the manner in which Homœopathists are occasionally made. The professional friend alluded to by Dr. S., is PROF. HENDERSON, of Edinburg, one of the most noted Homœopathists in Great Britain.

"Some eight or ten years ago, and old schoolmate of Dr. Simpson having begun business as a homœopathic druggist in Liverpool, kindly sent Dr. S. a present of a small box of homœopathic medicines, and a very beautiful painted box it was. During the time it was in Dr. S's. possession, he put it only to one use, viz., he gave it as an occasional plaything to his eldest son, who was then a child. The boy, revelling in his permitted amount of mischief, used in his sport to uncork the small bottles, empty their globules into a heap, and then refill the bottles from the general mass. Of course, this had speedily the effect of altering and disarranging the contents of the entire lilliputian drug shop; the globules pertaining to the different bottles were more or less thoroughly mixed together; and sometimes, when the child was tired of his occupation, others at last refilled the bottles from the general heap. A professional brother happening to call at Dr. S's. house one day when Dr. S. was absent from home, saw the box, and put it in his pocket. Many weeks afterwards, the new proprietor of the box met Dr. S., and told him that he had been trying to practice homœopathically, at which Dr. S. expressed his regret; and he added that he had seen some wonderful effects and cures from using the drugs contained in Dr. S's own former homœopathic box! Wrongly, perhaps, as Dr. S. now thinks, he did not at the time tell this physician that the globules of the bottles which he had been using were elaborately commixed; but the whole struck him as so good a joke at the moment, that he thought he would reserve it to bring it out upon his friend on some future and more ripe occasion, for the purpose of laughing him out of his homœpathic delusion. But, unfortunately, matters hastened rapidly on; the physician became more and more a homœopathist; and then it became too serious a matter to joke about, when he actually published a list of supposed homœopathic cures."

ADVANTAGES AND PLEASURES OF A PHYSICIAN'S LIFE.

From the eloquent Introductory Lecture of Prof. Knight we make the following extracts, and only regret that we have not space for more:—

"Another advantage of the medical profession is, that the mind of the physician is not, and if he is in any degree faithful to his duties cannot be, continually occupied with mere pecuniary matters He has a right, to be sure, to look forward to a fair remuneration for his services, and usually receives it; but his mind must be mainly occupied with other and higher interests. His duty to his patients, his anxiety for their recovery, his careful study of their diseases and of the means of relieving them, will engross his best and most diligent thoughts, and he will soon find that there are other books more interesting than his day-book and ledger. It is a misfortune attached to any employment, that its pecuniary results are its principal attraction. The business that is necessarily begun and pursued under the influence of the often-repeated and much-praised maxim, a penny saved is two pence clear, and a pin a-day is a groat a-year, cannot do much in elevating the mind, or ennobling the feelings, or in raising the man much above the earth on which he dwells.

"It is true that such employments may be, and often are, followed by the liberal-minded man, without self-deterioration; still their tendency is to belittle the mind, and to narrow the feelings into the compass of selfishness. I do not mean by this that a business is to be avoided as injurious merely because it is profitable; a gainful employment may be safely followed, so long as the occupation which it gives to the body and mind is its principal attraction.

"The mechanic may construct a machine in the hope of a large reward, and yet receive a far higher gratification from the successful exertion of his faculties, and this may be, and often is, the controlling motive of his labor rather than the pecuniary result. The merchant, while his gains are counted by thousands, may yet be more richly rewarded by the consciousness that his is the controlling mind of large and important interests, that he is successful in developing the resources of his country, and that he is ministering largely to the happiness of his fellow men. In all such cases, though accumulation may follow thrift, yet to accumulate is not made the chief purpose of life. From the temptation so prevalent in many other employments, to pursue gain with

greediness, the physician is guarded, by the full and constant preoccupation of his mind by other and higher thoughts, as well as by the early-learnt truth that such a pursuit will be unavailing. The physician who engages in his business with the purpose of becoming rich, uppermost in his heart, is very apt soon to leave it, and to become the nostrum monger, or the "pathy" follower, or to engage in some pursuit more congenial to his spirit, and more likely to gratify his desire. This freedom from temptation to petty gains, and resulting avarice growing out of the business of the physician, is one of its important advantages.

"Another advantage of the profession, and one which contributes largely to the happiness of the physician, is that it compels him to possess or to assume cheerfulness of disposition, kindness of demeanor, and a readiness to perform acts of beneficence. These constitute no inconsiderable portion of his stock in trade, and without a liberal share of them he will soon become bankrupt. He must be kind to his patients, considerate of their feelings, patient of their complaints though they may often seem to be unreasonable, and ready to afford them consolation and relief. He must therefore cultivate these feelings until they become a part of his very constitution. He who commences a course of this kind from the necessity of his position, will soon learn to continue it from the love of it; as the features which often called upon to express any one of the strong emotions of the mind, joy or sorrow, pleasure or pain, will become ultimately formed or moulded, so that what at first was transient, will become their habitual expression, so the mind into which any strong emotions habitually enter, whether of necessity, and more especially, when of choice, falls more and more under the influence of such feelings, until it becomes the controlling power of its actions. This in-working of the kind feelings which he is so often called upon to express and to experience, is so effective, that it is rare to find a physician advanced in life, who is other than a cheerful, social and benevolent man. The influence of this state upon his own happiness can hardly be over-estimated. It is a law of our nature as definite and as operative as the law of gravitation in its effects upon material bodies, that to do good to others, is to gain it for ourselves, and that our own happiness is very nearly in proportion to the active exertions which are made to promote the happiness of our fellow men.

"After all, however, the chief source of the physicians enjoyment springs from the successful result of his efforts to relieve distress, remove disease, and rescue the dying from their danger. As very much of his often disheartening anxiety and despondency is occasioned by the unfavorable termination of the cases submitted to his care, so his greatest joy grows out of those which end favorably. And it is to be remembered that by far the larger number of cases, even of dangerous diseases, end in recovery. There is a

pleasure, unappreciable except by experience, in the consciousness of power over disease and of ability to conduct it to a favorable end; there is joy that the life of one dear to many and perhaps important to the community, is saved; there is the feeling of gratitude fairly earned and often liberally bestowed. It is doubtful whether the physician ever feels a more honest and gratifying elation, than upon the recovery of his first patient from dangerous sickness. It is difficult to describe the emotion of the physician, who after long watching the progress of a dangerous sickness, first sees the light of hope breaking in upon the darkness with which it has been shrouded."

THE NECESSITY OF RE-VACCINATION.

Dr. G. Benedict, physician to the New York Northwestern Dispensary, closes an article on this subject, in the *New York Journal of Medicine*, with the following summary, the result of his experience in vaccinia as a preventive of small pox.

"That vaccination, properly performed, and repeated until the susceptibility to the vaccine disease is exhausted from the system affords entire immunity from the variolous disease.

"It may seem that, by including so much, my proposition is worthless, as it would extinguish not only the genuine disease, but its modification, varioloid. But we are to bear in mind that one two or three successive pustules may still leave the system unprotected, at least in part. Vaccination should be repeated until nothing like a pustule can be obtained. Let each one observe for himself, until evidence accumulates which shall sustain or overthrow this position; and let no one say that vaccination is not a protection for those in whom the susceptibility to variola is unusually strong, until he first ascertains whether there is not still left some susceptibility to vaccinia."

MAGNESIA IN POISONING BY ARSENIC.

An interesting case is related by Dr. Thomas R. H. Thomson, in a late number of the *London Lancet*, in which a woman took, by mistake, two scruples of arsenious acid. She was relieved by recently prepared hydrate of magnesia, of which not less than eight ounces were administered in the course of a few hours. The advan-

tages of this antidote are, that it is perfectly safe, easy of preparation, and mixed with facility. As very little of the poison, in this case was detected in the urine, the presumption is that it was rendered inert by its combination with the magnesia, and escaped by the bowels.

AMERICAN MEDICAL ASSOCIATION.

The Seventh Annual Meeting of the American Medical Association will be held in the city of St. Louis on Tuesday, May, 2d, 1854.

The Secretaries of all Societies in the Association are requested to forward to the undersigned, correct lists of their respective delegations *as soon as they may be appointed*,—and it is *earnestly* desired by the Committee of Arrangements that the appointments be made at as early a period as possible.

The following are extracts from Art. 2d, of the Constitution:—

"Each local Society shall have the privilege of sending to the Association one Delegate for every ten of its regular resident members, and and for every additional fraction of more than half of this number. The Faculty of every regularly constituted Medical College or chartered School of Medicine shall have the privilege of sending two delegates. The professional staff of every chartered or municipal Hospital, containing an hundred inmates, or more, shall have the privilege of sending two delegates,—and every other permanently organized Medical Institution, of good standing, shall have the privilege of sending one delegate.

"Delegates representing the Medical staffs of the United States Army and Navy shall be appointed by the Chiefs of the Army and Navy Medical Bureaux. The number of delegates so appointed shall be, four from the Army Medical Officers, and a like number from the Navy Medical Officers."

The latter clause, in relation to delegates from the Army and Navy, was adopted as an amendment to Article 2nd of the Constitution, at the last annual meeting of the Association held in New York, in May, 1853.

E. S. LEMOINE. St. Louis.
One of the Secretaries.

N. B. The Medical Press of the United States is respectfully requested to copy the foregoing.

Record of Medical Science.

The Influences of Chloride of Sodium on Absorption and Exhalation; a paper read before the Cook County Medical Society, by HENRY PARKER, *M.D.*

AMONG the most important remedial agents having a special influence upon the processes of absorption and exhalation, and the due interchange of these elements, I may mention Chloride of Sodium.

This substance, as the Society is well aware, is attracting the attention of medical men, as a remedy possessing peculiar and important powers in the treatment of certain forms and conditions of diseased action.

That a substance of such free and common use should possess the influences ascribed to it, may naturally excite the surprise and distrust of the ignorant and the charlatan; but the man of science, who sees in disease the absence of some element material and essential to the healthy body, or the presence of one unnatural and pernicious, will, when acquainted with its *modus operandi*, readily understand how it accomplishes certain good.

From some recent experiments, made by M. BERNARD, of Paris, upon the effect of this salt upon the blood, he ascertained that it greatly increased the capacity of this fluid for the absorption of oxygen.

Experiment.—Into two graduated glass tubes, containing each equal quantities of pure oxygen gas, he introduced equal quantities of blood freshly drawn from the jugular vein of a dog, without being exposed to external air. One of these tubes contained a small quantity of chloride of sodium. The blood in this tube absorbed immediately thirty-two parts of oxygen, while the blood contained in the tube, with only pure oxygen, absorbed only twenty parts.

Observe what a difference in the relative amounts of oxygen absorbed.

As this experiment of M. BERNARD was made upon blood out of the body, and without any special reference to the proportionate

amount of carbonic acid expired, I was desirous of ascertaining how far the internal administration of the salt, would, in its influences upon absorption, affect the exhalation of this excretion, carbonic acid.

With this view, I confined myself for a few days to a diet, plain, simple, and as free from *salt* as could well be obtained. During this interval, a given quantity of expired air was obtained from time to time, in a graduated glass tube of known capacity, over mercury, and transferred to a solution of caustic potassa.

The results of these repeated experiments may be expressed thus:

Whole amount of carbonic acid contained in each given quantity of expired air as indicated by the rise in the tube, 1.25 of a cubic inch.

The experiments were continued sufficiently long to furnish a reliable data, with which to compare future results.

Immediately upon concluding the above experiments, I took liberally of common salt, and especially upon going to bed at night.

The amount of carbonic acid contained in the same given quantity of expired air, obtained the following morning, and at the same hour as in some of the preceding experiments, viz. immediately upon rising from bed, was

Carbonic acid, 3.32 cubic inch.
Sp. G. of Urine, 1.035.

This experiment was repeated several times, observing the same diet, and, as in the former experiments, with this difference only, that I partook freely of salt, and with the same results as expressed above.

Observe what a material difference in the relative amounts of carbonic acid exhaled in each given quantity of expired air, as obtained from these two series of experiments—an increase in the latter over the former of one-hundred-and-thirty per cent.

It will be observed that these inquiries were made with special and only reference to the amount of carbonic acid exhaled, and not with any reference to the quantity of oxygen absorbed.

It is very probable, however, that in the process of absorption and exhalation, these two gases sustain certain definite relations with each other. What the proportion which exists between them may be, is not definitely determined. But that the increased absorption of one is attended by a corresponding increase in the exhalation of the other, I think is true. Blood may contain various gases; but it is evident that it never disengages them spontaneously. "They must always be extracted by displacement, in submitting to it some other gas, for which it may have a stronger affinity. This general rule is applicable to liquids,—these can only lose one of their component elements, by receiving a different one. Exosmose and endosmose, must occur." Bernard's experiment tends to establish the correctness of this, when applied to

the animal economy. Beneath the skin of a rabbit he injected a quantity of air. In due time this was analyzed, and found not to have diminished materially in quantity, but to have undergone a change. The oxygen only had been absorbed, and carbonic acid occupied its place. From these facts it is very evident that the entrance of one gas into the system is always attended by the corresponding exit of another. Hence the increased exhalation of carbonic acid observed in some of the above experiments—the result of an increased absorption of oxygen.

From the preceding inquiries into the influence of chloride of sodium upon the blood, we observed three important facts:

1st. An increase in the capacity of this fluid for the absorption of oxygen.

2d. An increase in the amount of carbonic acid exhaled. And,

3d. An augmented excretion of urea.

The effects observed upon these several processes are so marked that every intelligent and scientific physician must see the importance of their due consideration, in the treatment of certain forms of diseased action. With these facts before us, we infer that the use of this substance is indicated in those diseases, dependent upon, or accompanied by, a too small supply of the former, and an undue accumulation and retention in the system of the latter.

Does experience confirm this view, and the results of its use in such cases establish the correctness of our deductions? If we call to mind the pathology of those diseases, in which its use has been attended with the most marked benefit, we observe the existence of one or all of these three conditions, viz:

A diminished supply of oxygen, with an abnormal accumulation and retention in the system of carbonic acid and urea.

Congestive pneumonia affords us a good illustration of this,—a want of efficient oxygenation of the blood, and the due elimination of carbonic acid and those morbid substances designed for excretion. This want, oftentimes, if not generally, constitutes the only real source of danger to the welfare of the patient. That other means and other remedies may be demanded to subdue the local trouble, to remove the cause, of which this is an effect, I admit; but the period of time required to effect this is often protracted,—the patient sinking from a want of efficient oxygenation of the blood. This fact indicates the use of such remedy, combined with those adapted to the removal of the cause, as will most effectually promote the due interchange of these elements, viz. oxygen and carbonic acid.

Reasoning then from the known effect of chloride of sodium upon the blood, in promoting the absorption of oxygen and alimination of carbonic acid, we infer that, in this substance, do we find such a remedy.

And the fact, also, that the greatest apparent good results from

its use, when such pathological conditions exist, it seems to me, furnishes strong evidence that its *modus operandi*, or rather, *modus curandi*, is through its influences upon absorption and exhalation.

Does the experience of the Society in the use of this substance confirm this view?

In this connection, also, I might mention, as illustrating farther the *modus curandi* of the remedy under consideration, that class of diseases having a malarious origin, as exhibited in intermittent and remittent fevers. It is not my purpose now to attempt any explanation, or advance any theory relative to the exciting cause of these affections, further than to call to mind what has been advocated by the President of this Society and concurred in by others, viz:

That an important element, concerned in the production and development of miasmatic diseases, is "an atmosphere saturated with moisture, and constituted in part of carbonic acid, sulphuretted and carbonetted hydrogen, and other gases, such as deteriorate the air, both by their poisonous qualities and by their affinity for oxygen;" in a word, there is "defective oxygenation and an excess of carbonaceous material in the blood and tissues."

Now, if this be true, we can readily understand something of the *modus curandi* of chloride of sodium, when used in this class of affections. That it will arrest the paroxysms of an intermittent or remittent fever quite as effectually and promptly as quinine, all, I think, who have had experience with it as a remedy in such cases, will admit. Whence, then, this power? What the *rationale* of its operations? Evidently, is it not through its influence principally upon the blood, in promoting the ready absorption of oxygen, and the free elimination of those substances having a pernicious effect upon the economy? That it may exert *other* influences, and induce other and important changes in the system, all tending to bring about the same beneficial results, is unquestionably true, but as I purposed only to present a few facts relative to its importance as a therapeutic agent in promoting absorption and exhalation, I omit any further consideration of its *modus operandi* not immediately influencing this function.

Much additional evidence might be adduced in further support of the views presented, but as briefness in *society papers* oftentimes constitutes one of their *greatest* merits, if not their only redeeming quality, I submit the subject without further comment, asking for the facts presented and the deductions made, only that candid consideration from the Society which their value or importance may merit.—*N. W. Med. and Surg. Jour.*

On the Use of Cinnamon in certain Examples of Menorrhagia.—T. H. Tanner, M. D., Licentiate of the Royal College of Physicians; Physician to the Hospital for Women, &c., says:—Amongst the numerous cases of menorrhagia which come under my notice in hospital and private practice, instances are not unfrequently met with where no satisfactory explanation can be given for the increased catamenial flow. Of course, amongst these, patients suffering from general plethora, from hæmorrhage caused by polypi either within or without the uterus, or from that dependent upon fibrous tumors, uterine hydatids, ulceration (simple or malignant,) or upon a watery condition of the blood, are not included. In the instances alluded to, I have almost always been unable, after careful examination, to say upon what the menorrhagia depended, since no physical alteration could be detected in either the uterus or ovaries; it has only therefore been practicable to assert what were *not* the causes.

The symptoms usually presented are briefly these; the catamenia appear regularly every twenty-eight days, and are at first only of the proper quantity; but, instead of ceasing after a duration of three or four days, they continue unabated for ten or fourteen, and occasionally even for three weeks. The general symptoms which arise from this debilitating discharge are just as might be expected. There is general weakness, langor, mental depression, with pains in the head, loins, and so on; the patient suffering, it is to be remembered, not from any diseased condition, giving rise to the hæmorrhage, but merely from the loss of blood itself. In other instances the discharge continues a less time, but the flow is more abundant, clots being frequently discharged; this variety is generally followed by leucorrhæa. My own experience tends to show that these forms of menorrhagia occur more frequently in unmarried than in married women; but for many reasons, and especially because of the class of patients from whom my observations are for the most part deduced, I would not say positively that such is the case.

Ailments of this class are at all times troublesome to cure, but those I am considering are particularly so, from the absence of any special indications for treatment. In some of them, indeed, it appears, at first sight, only necessary to keep the patient quiet, to administer astringents, especially such as act particularly upon the uterus, and to regulate the diet, in order to give the desired relief. But it will often be found that these means are quite inefficient, and that the acetate of lead, gallic acid, the ergot of rye, oxide of silver, sulphuric acid, tincture of sesquichloride of iron, and similar remedies may be employed without any avail. In thinking over these cases I was led, from a remark made by Dr. Pereira, to try the use of cinnamon. In the last edition of the "Elements of Materia Medica," this gentleman says, when speak-

ing of cinnamon, that "some writers regard it as acting specifically on the uterus," (vol. ii. p. 1307,) and reference is then made to the writings of Sundelin and Wibmer. As far as I know, these are the principal authors who have recommended the use of this agent; for I am not aware that mention is made of it by gentlemen who have written on obstetrics and the diseases of women in this country. Having thus been led to try the effects of this agent and having derived the most beneficial effects from its employment, I have felt desirous of making its value more generally known, that its utility may be tested on a large scale.

That its beneficial action is really due to some specific effect which cinnamon exercises upon the uterus, and not any astringent property it may possess from the tannic acid which the bark and leaves of it contain in common with all the lauraceæ of plants, is, I think, certain; and partly in confirmation of this view, it may be mentioned, that in a case of labor in which I employed it, it *appeared* not only in a marked manner to increase the severity and rapidity of the pains, but the patient, who had in her previous labours suffered severely from flooding after the birth of the placenta, on this occasion lost only a very small quantity of blood. That this fortunate circumstance was due to the administration of the cinnamon I do not of course pretend positively to assert; the case is merely mentioned to give some color to the opinion expressed that this agent acts specifically upon the uterus. Much clearer evidence of its value in such cases of menorrhage as I have described, is, however, easily obtained. The principal points I would now refer to, are, that it acts after the failure of other astringents; that it is most efficacious given alone, uncombined with other medicines; that if its employment be discontinued too soon, the discharge of blood returns; while, in a few instances it has been found necessary, after an apparent cure, to resort to its employment for the two succeeding catamenial periods, when the menstrual flow, after continuing for three or four days, has not given any signs of abatement, and when the patient has begun to suffer from mental depression and the early symptoms of general debility. The form usually employed, and which appears to be the best, is that of the tincture, in drachm doses, using cinnamon water as the vehicle; it should be taken about every six hours, but not more frequently, as it is apt to give rise to nausea and vomiting. It is also better to continue its administration for about fourteen days after the symptoms which called for its employment have disappeared; and even then, if the case has been an obstinate one, a draught composed of it should be taken once daily for a month.

In concluding these brief observations, I trust I shall not be understood as recommending cinnamon as an infallible remedy in all examples of menorrhagia. All I would wish to assert is, that

in a certain class of cases I have found it to possess properties of great value; and though I might have waited until I had been enabled to test its properties on a larger scale, yet I preferred giving the results of my experience at once.—*London Lancet.*

On some of the Signs of Incipient Pulmonary Phthysis.—M. Bourdon says:—It is known with what difficulty the presence of pulmonary tubercles is diagnosticated when the disease is in its incipiency, and how insufficient the stethoscopic signs are. The latter may be wanting when the lungs contain a large quantity of tubercles; they may exist when the lungs contain none; and they may be masked by different sounds. The general symptoms are not a sure means of detecting the disease; a simple catarrh may be attended with very grave symptoms, while a true phthisis may affect the general system very slightly. It is at the commencement of the disease that the diagnosis is most important, because this is the only time that there is a chance of treating it with any success. M. Bourdon has sought to elucidate this obscure point. Attracted for a long time by the frequency of certain symptoms in phthisical patients, he has studied them with care, in order to establish their value as signs. These symptoms are arranged under many heads, according to the organs in which they show themselves:—1. Gastric symptoms. 2. Hepatic symptoms. 3. Thoracic pain. The gastric symptoms are not to be confounded with the morbid phenomena observed in almost all diseases. Those which coincide with the incipiency of phthisis are nauseas, vomitings, epigastric pains, dyspepsia. You must not wait to find these symptoms in every case; nevertheless, they are extremely frequent, since M. Bourdon found them in more than two-thirds of his subjects. Some phthisical patients suffer from one of these symptoms; others, on the contrary, have them all combined. Nausea and vomiting are most common; and, it is generally after the cough that they exhibit themselves. It must be observed, however, that their frequency or intensity are not at all proportioned to the cough; they occur when the stomach is empty as well as after a repast. Generally the vomitings consist of mucous matter, rarely of bile. Still, while they show themselves with a large number of the tubercles, they do so in a positive manner with very few. Another very common symptom is the epigastric pain. In the largest number of cases, it is accompanied with nausea and vomiting; nevertheless it occurs alone sometimes. With the largest number of patients it is only manifested upon pressure, or is so slight as not to be noticed by the patient until their attention is attracted to it; in a smaller number of patients it manifests itself spontaneously. Although it shows itself both

before and after eating, it is yet most commonly observed after the ingestion of food. Long continued dyspepsia, without nausea or vomiting, is frequently observed. Frequently the gastric symptoms precede the thoracic; but often also they show themselves at the same time or subsequently. Do they imply a lesion of the stomach? is a question which has occupied the author. Sometimes he found the stomach healthy; most frequently it presented some lesion, which was oftenest a papillated state of the mucous membrane. The author thinks the pressure of the tumefied bronchial glands upon the pneumo-gastric might contribute to the disorders of the stomach.

2. The liver sometimes presents remarkable peculiarities. In half the patients examined by M. Bourdon there was an increase of volume, frequently attended with pain. The increase of volume seemed to be particularly observable in the right lobe; the consistence of the organ seeming to undergo no change. The changes of sensibility are less frequent than the changes of volume. Spontaneous pains are rare; they only manifest themselves under pressure, or under some sudden and straining movement. In some rare cases this change of sensibility is observed without change of volume. As to the change the liver undergoes, the autopsies prove that in more than half of the cases it is due to fatty degeneration more or less advanced. The bile is generally of greater density and more highly colored.

3. The largest number of phthisical patients suffer from pain between the shoulders and in the sides of the chest; but, besides these spontaneous pains, there are others to which M. Bourdon calls the attention of physicians, and which are only recognized through the medium of percussion. The points where these pains are discovered are numerous, but they are most often seated below the clavicles, and only on the side affected with tubercles. If both sides are affected the pain is on the side most affected. As to the cause of this pain, M. Bourdon, like M. Beau, is inclined to ascribe it to intercostal neuralgia.

The conclusions drawn by M. Bourdon from his researches are as follows:—When prolonged dyspepsia, nauseas and vomitings are observed, without any assignable cause; when the liver is abnormally developed, without there being hepatitis or disease of the heart; when these phenomena are exhibited independent of any other affection, or in the course of chlorosis, or after measles or typhoid fever; the physician should turn his attention to tuberculosis, examine the patient with great care, and even when there is doubt of the reality of the pulmonary affection, he ought to act as if convinced, or at least he should take his precautions.—*Archives Gen.*—*Virginia Med. and Sur. Journal.*

A Man who has Slept Five Years.—We called yesterday to see the man who has been asleep for five years, and whose case was detailed in the *Times* some weeks since. We found him in what seemed like a sound sleep. He was lying in bed, his eyes nearly closed, his respiration rather slower than usual, his breathing a little stertorious, pulse some seventy-five strokes in a minute, soft and weak. On attempting to open his eyes, he firmly closed them, and when, by force, the lids were opened, the eyes were rolled upward, so that it was impossible to see the pupils. The mouth was slightly opened; on attempting to open it wider, the jaws were instantly locked. There was a constant tremor of the eye-lids, and from the mouth there was some drivelling. His body was extremely emaciated; his arms were folded upon his breast, and any attempt to remove them was strongly resisted. The muscles seemed rigid and tense when the effort was made, and, indeed, it was impossible, without violence, to change at all the position of his limbs. Once during our stay, he drew a long breath, like a man who is about to turn in his sleep. At another time, he hitched himself up a little in bed. He was lifted up bodily and seated on the side of the bed; his head was still bent forward on his chest, his legs crooked under him at the same angle, and his arms folded as when he was lying. There was nothing to indicate that he would not retain the same position for weeks. We lifted one foot, the other came up with it. There was little or no bending at the knee or at the hip, the feet were raised only as the upper part of the body was carried backwards. He was placed standing upon the floor. It required a few moments to balance him exactly, after that he stood in the same position so long as we remained, there was nothing to indicate that he would not maintain the same posture for a month.

This certainly is a most marvellous case. There is not the slightest chance for any collusion or deception in the matter. Many of our best physicians have examined him—none, so far as we can hear, believe any deception in the case to be possible. From physicians in the western part of New York, and from men of the highest standing, we are assured that the story which is told of him is perfectly true.

His name is Cornelius Vroman, he was borne in Schoharie county, but has lived, since he was seventeen years of age, in Clarkson, Monroe county, not far from Rochester. He was a hard working man, a good worker, temperate, trusty, and, at the time when his strange sleep came on, he was working on the farm of Mr. Moses Jennings. His mother is dead, he has a father and two brothers living in Clarkson. On the 19th of June, 1848, he felt unwell enough to call in Dr. John S. Cole, who found him complaining of some pain in the stomach and in the head, for which he prescribed. After this, without becoming any more sick,

his sleep each night grew longer, until at last it was found impossible to wake him. Out of this sleep he has never come, to remain wakeful for more than sixteen hours at a time, and the aggregate of all his waking hours since the seizure is not over three days. At first they were oftener, but now the waking intervals recur about every six weeks. The last time he awoke was while he was in Rochester, some ten weeks since, which gives us a hope that his waking hour now approaches, and that we may see him in his wakeful condition. When awake, he seems totally unconscious of his peculiarity, and has said some things which indicate that he remembers matters as they were before his change. They say that he straightens himself up then, and walks as limberly as others. Yet now, to handle his limbs, we fear that they must be partially anchylosed. But on this point we are not satisfactorily informed.

His diet consists principally of milk, sometimes with a little bread soaked in it. It is with some difficulty that it can be administered. The jaws must be forced open, as in tetanus, and the liquid poured in between his teeth. Once, he went without any food for five days, but his friends objected to any farther conduct of the experiment, though there was no change in his symptoms during that time. When the seizure occurred, he is said to have weighed 160 pounds, now he cannot weigh over 90 pounds. His height is six feet two inches. The secretion of the kidneys is discharged once or twice a day, it is very high colored, and not much diminished in quantity. Possibly it is from habit, possibly from some remains of consciousness, that in this matter he is subject to the wishes of his attendants. The alvine evacuations are very scanty, occurring not oftener than at intervals of from six to twenty days.

Once he was left standing for three days, there was no change in his position during that time.—*N. Y. Times.*

This individual has since died.—Ed.

A Curious Case of Fracture above the Third Vertebra Cervicalis.—James W. Claiborne, M. D., of California, relates the following:—Daniel Edson, of Fall river, Massachusetts, aged twenty-seven years, fell from a stage about the 10th day of July 1853. The horses were running at full speed, and as he fell from the driver's seat, he thinks he struck his neck on the tongue or pole of the vehicle.. Reaction was slow in establishing itself, and there was such a cadaverous pallor and collapse, that in connection with other symptoms, his recovery was regarded as hopeless. He maintained a horror of moving from one side to another, was speechless, and desired to be kept in a state of absolute rest for a

week or ten days. As soon as the neck was examined, a hard protuberance of osseous firmness was perceived at the back of the neck, on a level with the lobulus pendulus of the ear. It was situated a little to the left of the spinous processes, and under the tendinous origin of the trapezius. It has never increased or diminished in size, and is about the size, of a split walnut. The protuberance is situated in the line of the hair, below which, according to Velpeau, (Anatomy, vol. i, p. 203,) the atlas is seldom situated. It is therefore fair to say, the tumor is above the third cervical vertebra.

There was no difficulty in the breathing, or injury to the phrenic nerve, as might be supposed. The inferior maxilla was for several weeks firmly closed, and it was almost impossible to introduce a spoon between the teeth. The attempt would cause him great pain at the site of the node or protuberance, and an apprehension of danger in the effort. As soon as he was so far recovered as to attempt walking, for many days he would carry his head steadied with a hand over each ear, and at present, some three months after the occurrence, he has but few of the motions of the head. The motion backwards is impossible, and it is only very recently that he has attempted to look down at his feet. With regard to rotation, he turns the head easily towards the tumor which is on the left side of the spine, but it causes him much pain to turn toward the right. Swallowing was greatly interfered with, and for some time after the event he was often troubled with a feeling of there being a small substance lodged in the pharynx. He further informs me that some few weeks ago he received a blow or jar upon the crown of the head, and that he felt a grating or crepitation a little below the occipital protuberance of the right side at that time.—*Va. Med. Gaz.*

External Stimulus in Cholera.—The resources of professional judgment in the treatment of Cholera are not so numerous and efficient as to render a somewhat novel remedy unacceptable to those whose lot it may be to undertake the charge of many cases. I am indebted to a lady of heroic mind and great intelligence for the hints which led me to adopt in the collapse of cholera the powerful external stimulus which I shall presently describe. It appears that in the most desperate cases, even when life has appeared extinct, the native Indians are accustomed to apply the actual cautery freely to the abdomen, not unfrequently with the happy result of restored vitality. The remedy, based on the same principle, which I have employed in three cases with complete success in the borough jail of Newcastle, is precisely similar in kind, though somewhat less harsh and formidable in degree. A

piece of linen dipped in brandy is placed over the epigastrium or abdomen, and ignited; the brandy burns away in a minute or so, producing a considerable feeling of pain, which renders it necessary to secure the hands of the patient. Slight vesication will probably follow, and, if successful, in a short time heart and pulse begin to return, and the feelings of the patient are greatly improved; vomiting will also generally be put a stop to. It is probable that the application may soon require repetition, the situation being somewhat varied. In one case, now convalescent, in which death was apparently close at hand, the most complete effect was produced by a third application along the spine in the lumbar region. Time will not permit me to give details of cases, but having now tried the "brandy blister" in seven or eight cases of total collapse, I repeat that in three it has been entirely successful, and in others has had the effect of temporarily rousing the patients; and if, as experience has taught me, in some of these it had been repeated with more energy, greater success might possibly have resulted. The patients now convalescent say that it is a severe remedy, but are quite conscious of its beneficial effects, and attribute to its use, without any hesitation, their restoration from impending death.—*Mr. Greenhow in London Lancet.*

Mortality from Cholera influenced by Elevation.—It was discovered during the epidemic cholera of 1848–9, in London, that the rate of mortality by that disease was nearly in the inverse proportion of the elevation of the ground on which the dwellings of the inhabitants stood. The same relation between the rates of mortality at different elevations, though the deaths have been comparatively few, has hitherto been observed in the present epidemic. The mortality from Cholera in the districts at an average elevation of less than 20 feet above Trinity high-water mark has been 31 in 100,000 inhabitants; in the districts of an average elevation of 20 and below 40 feet (20—40 feet) the mortality has been 16 in 100,000; at an elevation of 40—60 feet the mortality has been 11 in 100,000; at 60—80, it has been only 4; at 80—100, only 3. Marylebone, at an average elevation of 100 feet, is the only exception to the law; the mortality has been there 13 in 100,000. At Hampstead, where the elevation may be put at 350 feet, there has hitherto been no death from Cholera. Exceptional circumstances disturb the average in particular districts; but it is a general rule that the danger of dying of cholera, and of all plagues, diminishes within certain limits in proportion as the dwellings of the population are raised above the level of the sea. —*Med. Times and Gazette.*

On the Local Application of the Vapor of Chloroform.—In an article in the *Dublin Quarterly Journal*, Dr. Hardy gives several examples of the application of the vapor of chloroform to the vagina and neck of the uterus, in painful affections of these parts, by means of an instrument which he has invented for that purpose. The following are his conclusions:—

"In observing the effects of chloroform as applied locally in the form of vapor in the above cases, I have endeavored to obtain as correct a notion of it as possible, in order that a true estimate might be arrived at of its value as a remedy. Besides the cases here recorded, I have applied the vapor locally in various other forms of irritation. One of these in particular, I was anxious to know its action in—namely, *pruritus pudendi*, a disease exceedingly troublesome and unpleasant to the patient, and for the relief of which she is often very reluctant to ask a remedy until forced to do so. I have used it in a case of this kind in the person of a very intelligent patient, who for a length of time had been annoyed, particularly on the approach of a menstrual period, by this distressing complaint, for which she had made use of various remedies. The vapor of chloroform, she informed me, afforded her relief from her uneasy sensations. On referring to one of the cases (Case v.) detailed, it will be seen that there was a very severe sense of scalding in the vagina, which seemed to depend a good deal on uterine irritation. Knowing the heat caused by the vapor of chloroform, I feared this patient should have suffered severely from its application; but, on the contrary, she was quite relieved of it; so in *pruritus pudendi*, arising from a similar cause, the like results have been obtained as in her case.

"If future investigations as to the effect of the vapor of chloroform when locally applied coincide with the results already observed in the series of cases herein detailed, it seems reasonable that the following conclusions be considered deducible:—

"*First.* That in many forms of disease attended with pain or irritation, the local application of the vapor of chloroform will frequently act as quickly in affording immunity from suffering as though inhaled in the usual manner.

"*Secondly.* That the vapor locally applied is not attended with any unpleasant effects (save the sensation of more or less heat) either at the time or subsequently, and is therefore eligible under circumstances contra-indicating its use by inhalation.

"*Thirdly.* That as a remedy its local application is preferable to the use of opium and most narcotics in spasmodic and painful affections, particularly of the uterine system, owing, first, to its freedom from causing derangement of the digestive organs, and, secondly, to its greater rapidity of action."—*N. Y. Jour. Med.*

Chloroform in Hypochondriasis.—At the meeting of the College of Physicians in Ireland in June, Professor Osborne stated that he had lately, in two cases, opportunities of observing a peculiar effect of chloroform taken into the stomach, in controlling the depressing and saddening feelings belonging to hypochondriasis. Considering that state to be produced by a depraved sensibility of the stomach or colon, and frequently of both, he was led to the internal employment of chloroform, which, being promptly volatilized at the temperature of the stomach and before being decomposed by the process of digestion, ought to be expected to act as a local anæsthetic, even though the dose should not be sufficient to produce any change in the function of the brain.—*Dub. Quar. Journal.*

Veratrine in Rheumatism.—Some marvellous results have followed the use of veratrine in articular diseases. The trial was made by M. Trousseau, who gave the tenth of a grain in a pill the first day; two on the second; three on the third, and so on increasing the dose, one pill each day, until it reached six or seven. Its action appears to be most decided in breaking down the inflammatory action. It is maintained that it has anti-rheumatic virtues and from its influence over the pulse it may be found by further trial, to be a most valuable therapeutic agent in all these arthrodial affections.—*Iowa Medical Journal.*

Felt Splints; *by* F. H. Hamilton, *M. D.*—Some years ago, I think in 1845, felt splints were brought to me by an agent of the manufacturer. The felt was sold in sheets, and also in pieces, modelled so as to be readily adapted to the form of the limbs. In some respects it was superior to gutta percha, and I am inclined to think that on the whole it was the best splint ever used. I cannot learn that these splints are now manufactured in any part of the United States, and I will therefore enclose you the recipe for making them, which was kindly given me by the agent, and which I have frequently used myself:—Dissolve three pounds of gum shellac in two quarts of alcohol. It should be dissolved in a tin vessel, furnished with a tight cover to prevent evaporation.—Spread a piece of old or new woolen cloth on a board, and with a clean brush saturate both sides of the cloth with the solution. Hang it up until it is thoroughly dried. Lay it again upon the board and apply a second coat of the solution to one side only of the cloth. Dry again, and apply a third coat to the same side. There will now be three successive layers upon one side and one

on the opposite. While the last coat is yet fresh, fold the cloth so that the side having three coats shall be applied to itself. Now, with a hot flat-iron, smooth and press the surfaces together. When it is cold, a slight rubbing with sand-paper makes it fit for use. It becomes a firm, almost unyielding board, but exposure to a moderate heat will make it pliant, so that it can easily and accurately be adapted to any surface.—*Buff. Med. Jour.*

Poisoning by Atropia applied to the Conjunctiva.—The *Gazette des Hopitaux* gives an account of a case of poisoning by atropia, which is remarkable for the small quantity employed, and the place of application, vix: the healthy conjunctiva. A patient in the Hospital, Saint Antoine, was laboring under double cataract complicated with adhesions of the iris to the lens. In order to ascertain exactly the condition of the eyes, three or four drops of a solution, containing about one grain of atropine to two ounces of water acidulated with acetic acid, were instilled into each eye. Half an hour afterwards the patient suffered from vertigo, and complained of disagreeable, unusual sensations; three quarters of an hour later he manifested all the symptoms of poisoning by belladonna,—redness and animation of the face—pupils, though irregular, enormously dilated, incessant hallucinations, seizing the bedclothes, and grasping at objects in the air. He could not raise himself from his bed, nor advance a step without being supported, as his limbs trembled and gave way at every effort. Pulse full, and beating one hundred and twenty. These symptoms gradually passed off, but the patient did not recover his normal condition for three or four days afterwards. Being interrogated upon the delirium and hallucinations of the preceding days, he had only a vague recollection of what had passed.—*Dublin Medical Press.*

Obituary Notice of Arago.—A life of so much labor had worn down his health. Although attacked with diabetes, he still contemplated putting the last touch to his unfinished works. Bright's malady set in and aggravated his situation, which was complicated with dropsy of the abdomen, attended with effusions, and swelling of the extremities. All announced his approaching end; yet his mind was not for a moment obscured. Shortly before his death, although blind, he superintended in some difficult researches; he asked M. Babinet to prepare for him a table of more accurately determined numbers for the lengths of undulations, that he might bring to completion a memoir on interferences; and he finished the editing of his Physical researches on the Planets, &c.,

&c. He died in the midst of these arduous occupations, on the 2nd of October, at the age of sixty-seven years and a half, a few minutes after having shaken the hand of M. Biot.

We have mentioned some of the works which Arago accomplished in his younger days. These works were completely eclipsed by the discoveries to which his name has since become attached, which embrace the following principles:

1. The discovery of chromatic and rotatory polarization.
2. That of Electro-magnets.
3. That of the magnetism which is developed when bodies are revolved near a magnet.

Arago was an Encyclopedic genius. Science, Literature, Political and Social Economy, his vast intelligence embraced all with equal ability. His powerful faculty of assimilation, popularization, and of application of principles, placed him everywhere in the first rank. Whether Orator or Professor, he shone with brilliancy both in political and scientific assemblages. He was distinguished for the perspicuity and elegance of his style, and occupies an eminent place among the prose writers of France.

In the midst of so much grandeur, Arago led a most modest life. He considered as lazy whoever did not work fourteen hours a day; and such days were for him days of repose. Although so absorbed with his occupations, he still found time to appear in the society of Paris as one of its most spirited conversationists.

While devoted to continued labor, he completely forgot his own interests, and had only what was barely necessary for the support of his family.—*American Journal of Science.*

Dysmenorrhœa.—In Germany the bi-borate of soda is used in conjunction with ergot, to produce uterine contraction, and alone to facilitate the excretion of the menstrual fluid. Dr. Bennett, of London, regards it as an important agent in fulfilling the latter indication, and employs the following formula in many cases of dysmenorrhœa:—℞. magnes. carbon., gr. v.; pulveris rhei., gr. ij.; sodæ biboratis, gr. x.; infusi columb., ʒj.; *ter die summend.*—*Med. Times and Gazette.*